ADDRESS

DELIVERED AT

THE ANNIVERSARY MEETING

OF THE

GEOLOGICAL SOCIETY OF LONDON,

On the 18th of February, 1876;

PREFACED BY

THE ANNOUNCEMENT OF THE AWARD

OF

THE WOLLASTON MEDAL,

THE PROCEEDS OF THE DONATION-FUND,

THE MURCHISON MEDAL

AND GEOLOGICAL FUND,

AND

THE LYELL MEDAL AND FUND

FOR THE SAME YEAR.

By JOHN EVANS, Esq., F.R.S.,

PRESIDENT OF THE SOCIETY.

LONDON:

PRINTED BY TAYLOR AND FRANCIS,

RED LION COURT, FLEET STREET.

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PROCEEDINGS

AT THE

ANNUAL GENERAL MEETING,

18TH FEBRUARY, 1876.

AWARD OF THE WOLLASTON MEDAL.

The Reports of the Council and of the Library and Museum Committee having been read, the President, JOHN EVANS, Esq., F.R.S., presented the Wollaston Gold Medal to Professor HUXLEY, F.R.S., F.G.S., addressing him as follows:—

Professor HUXLEY,—

It is a source of great satisfaction to me that it should fall to my lot to place in your hands the Wollaston Medal, which has been awarded to you by the Council of this Society in recognition of your distinguished services to geological science.

Those services have been so great and are so universally acknowledged that it seems hardly necessary to dilate upon them. For a period of upwards of five-and-twenty years you have been engaged in biological researches, which have resulted in throwing a flood of light upon the structure, affinities, and development of organisms of every class, from those so simple as to occupy the border territory between the animal and vegetable kingdoms, up to the highest forms of mammalian life.

Such researches cannot but have had a great and beneficial influence on geological thought. But your services to geology and to this Society are of a far higher and more direct character. Not only have you furnished to our 'Proceedings' numerous and valuable palæontological essays, but on three occasions, either as President or as representing the President of this Society, you have delivered Anniversary Addresses which are models of the philosophical exposition of great geological principles, such as I sincerely wish it had been in my power this afternoon to imitate.

In addition to these services to the Society, you have, it may

safely be affirmed, done more by your lectures than almost any other man to advance palæontological studies among those who are undergoing a course of scientific training, while to more popular audiences you have, by your vivid and lucid descriptions, rendered intelligible those marvellous natural processes by which such beds as our Limestones, Coal, and Chalk have gradually been built up.

As formerly one of your Secretaries, I know the deep interest you take in all geological pursuits; and I therefore venture to express a hope, in which all in this room will share, that the day may not long be distant when, with renewed health and strength, and a greater amount of leisure at your command, you may again be able to take a frequent part in the meetings of this Society, of whose appreciation of your labours this medal is the symbol.

Professor Huxley, in reply, said:—

Mr. President,—

I am so much more accustomed to the language of criticism than to that of panegyric, that I feel a certain difficulty in framing a reply appropriate to the address with which you have just honoured me.

To be enrolled among the eminent men who have been recipients of the Wollaston Medal is a distinction of which the most ambitious aspirant to scientific honours may be proud. The terms of personal kindness in which you have clothed the award of the Council, and the warmth of my reception by the meeting, lead me to hope that I may, in addition, regard the distinction which has been conferred upon me as a mark of the goodwill of the colleagues with whom in past years I have been so closely associated.

It is my hope and expectation, Mr. President, that the wish which you have so kindly expressed as to the resumption of my palæontological work, will be fulfilled.

The great biological question of the day is the problem of evolution; but geologists, as Kant says, are the archæologists of nature, and the sole direct and irrefragable evidence of the method whereby living things have become what they are, is to be sought among fossil remains. If I have in any degree merited the unexpected honour you have conferred upon me, it is because I have steadily kept this truth in view; and if I shall ever succeed in deserving the Wollaston Medal better than at present, it will be by further attempts to translate the archæological facts of nature into history.

Award of the Wollaston Donation-fund.

The President then presented the Balance of the proceeds of the Wollaston Donation-fund to Mr. J. Gwyn Jeffreys, for transmission to Professor Giuseppe Seguenza, of Messina, F.C.G.S., and addressed him in the following terms:—

Mr. Gwyn Jeffreys,—

In placing in your hands the Balance of the proceeds of the Wollaston Fund for transmission to our foreign correspondent, Professor Seguenza, of Messina, may I request you to convey to him at the same time our high sense of the value of his investigations upon the Tertiary beds of Italy and Sicily, on which he has already published such numerous and important memoirs.

Will you, in addition, express a hope that this mark of our appreciation may also prove of some assistance to him in the further prosecution of his researches.

Mr. Gwyn Jeffreys, in expressing the thanks of his friend Prof. Seguenza, said that this testimonial would not only be highly valued, but be especially acceptable, because the stipend of an Italian Professor was too small to enable him to prosecute his palæontological researches as fully as he could desire.

Award of the Murchison Medal and Geological Fund.

The President next handed the Murchison Medal to Professor Ramsay for transmission to Mr. A. R. C. Selwyn, F.R.S., F.G.S., and spoke as follows:—

Professor Ramsay,—

I place in your hands the Murchison Medal and the portion of the Murchison Fund which have been awarded to Mr. Alfred R. C. Selwyn, F.R.S., in recognition of his services to Silurian geology; for no one can appreciate better than yourself the character and importance of his work, or can better convey to him the assurance of the high estimation in which it is held.

As one of the officers of the Geological Survey of this country, engaged in unravelling the intricate Lower Silurian Rocks of North Wales with their associated volcanic deposits—as afterwards in charge of the Geological Survey of Victoria, mapping its Silurian

strata, its gold-bearing rocks and auriferous gravels of different ages, and tracing the relations of the latter to the Miocene beds of the colony, and to the older rocks—as subsequently the successor of Sir William Logan in the direction of the Geological Survey in our North-American territories from the Atlantic to the Pacific Ocean, the labours of Mr. Selwyn have extended over an enormous field.

How successfully he has worked in it, the numerous and excellent maps and sections executed under his superintendence, and his various reports and papers fully testify.

There is something peculiarly appropriate in the Medal founded by Sir Roderick Murchison being given to one whose labours, like his own, have lain so much among Silurian and other Palæozoic rocks, among gold-fields, and in the direction of Geological Surveys. While it marks our appreciation of Mr. Selwyn's services to Geology, it will, I trust, not be the less welcome to him as a proof that though absent he is not forgotten by his fellow workers.

Professor Ramsay, in reply, spoke as follows:—

Mr. President,—

On behalf of Mr. Selwyn, I return thanks for the honour that has been conferred on him. When Mr. Selwyn joined the Geological Survey of Great Britain many years ago, I may almost say that he received his first lessons in the art of Geological Surveying from me; but he very soon proved himself so proficient in the work, that a large part of the survey of the intricate Lower Silurian rocks of North Wales was executed by him in a masterly manner.

When the Geological Survey of the Colony of Victoria was established by the Colonial Government, Mr. Selwyn was selected to begin and conduct that work; and there also, with the help of Mr. Daintree, the Browns, and others, it was his lot again to be engaged on a great scale in unravelling the intricacies of Silurian geology. After that Survey was abolished, no one was considered so fit as he to succeed the late Sir Wm. Logan as Director of the Geological Survey of our North-American dominions; and there is therefore an appropriateness in the award of the Murchison Medal to one who has done so much excellent work among Silurian rocks in three regions of the world.

In presenting the Balance of the Murchison Geological Fund to Professor Ramsay for transmission to Mr. James Croll, the President said:—

Professor RAMSAY,—

Will you convey to Mr. Croll the Balance of the proceeds of the Murchison Fund, and at the same time express the hope of the Council of this Society that it may prove of service to him in the prosecution of those studies with which his name has been so long and so honourably associated.

His researches on Ocean Currents, on Glacial Phenomena, on the bearing of the latter on Geological time, and of both upon Climate, were generally known and appreciated, even before the appearance last year of his work on Climate and Time, in which the results of his studies are so carefully and ably expounded.

The author of that book would be the last to regard the subjects of which it treats as being all now definitely settled, and requiring no further investigation; and it is in the hope that his inquiries into the phenomena of glaciation, and into the physical causes conducing to extreme modifications of climate may be still further prosecuted, as well as in recognition of the valuable past labours of Mr. Croll, that the Fund which I place in your charge has been awarded to him.

Professor RAMSAY, in reply, said:—

Mr. PRESIDENT,—

In returning thanks on behalf of Mr. Croll, I have no need to enlarge on the merits of a man so well known to geologists by his numerous memoirs, and now especially by his remarkable work, 'Climate and Time;' and though on a range of subjects so wide it is not to be expected that there should be no opponents to some of his views, there can yet be no doubt that the ability which he has displayed commands the universal respect of men of science and the adherence of not a few.

AWARD OF THE LYELL MEDAL AND FUND.

The PRESIDENT then handed to Professor MORRIS, F.G.S., the first Lyell Medal and the entire Proceeds of the Fund, and addressed him in the following terms:—

Professor MORRIS,—

This is, as you know, the first occasion on which the Award of the Medal and the Proceeds of the Fund so liberally bequeathed to the Society by our illustrious Fellow, Sir Charles Lyell, has been under

the consideration of the Council. It has, I may venture to say, been unanimously felt that it was impossible to find a more worthy recipient than yourself, and that, in awarding both the Medal and the available proceeds of the Fund to one whose name was so intimately connected with the progress of Geological science, we were best carrying out the intentions of the founder, and attaching an importance to the Medal which would show that, though founded later, it takes equal rank with the other medals at the disposal of the Council.

You have been a Fellow of this Society for upwards of thirty years; but the published results of your geological investigations extend over a period of more than forty. Your 'Catalogue of British Fossils' has long taken rank as a standard work, while your communications to this and other Societies, both on Geological and Palæontological subjects, have been of high value and importance. Your lectures have done much to spread a taste for Geology, and to enlarge the number of its students; and those who have heard you take part in our discussions must have been astonished alike at the minuteness of your knowledge of every branch of Geology and Palæontology, and at the powers of memory by which you were enabled to apply it.

I have much pleasure in handing the Lyell Medal to one who, like its founder, has rendered such long and meritorious services to our science, and am glad that, under the wise and liberal provisions of the bequest, this token of our appreciation is accompanied by a sum which may prove of assistance in enabling you to carry still further your valuable observations and researches.

Professor Morris replied as follows:—

Mr. President,—

In receiving at your hands the first award of the Medal founded by Sir Charles Lyell, I cannot but deeply feel the distinguished honour which the Council of the Geological Society have this day conferred upon me. It is unnecessary to advert at this meeting to the works of an author whose labours in the field he earnestly cultivated have so greatly enlarged the bounds of geological knowledge and influenced the tone of geological thought. I may say, Sir, that it was chiefly by reading the first edition of Sir Charles Lyell's 'Principles of Geology,' and his review in 1827 of Poulett Scrope, that I was led to perceive their philosophical bearings, and was stimulated to continue geological research; for although the 'Theory of the Earth' had been illustrated and supported by the eloquence of a Playfair, it was the

indefatigable activity of Lyell, and his constant accumulation and assimilation of new facts, that led to the general acceptance of the principles advocated by Hutton. Hence, whilst I cannot but feel strongly impressed by the kind appreciation of the Council in this award as a recognition of my sincere but imperfect attempts to assist the progress of geological science, the gratification I experience at this unexpected honour is greatly enhanced by the consequent association of my name with that of Lyell.

THE ANNIVERSARY ADDRESS OF THE PRESIDENT,

JOHN EVANS, Esq., F.R.S.

In accordance with the custom of this Society, my first duty on the present occasion is to place before you some account of those whom the busy hand of death has removed from among our ranks during the year which has elapsed since our last anniversary. Perhaps on no former occasion have we had to mourn over losses more numerous and, indeed, in many cases, so irreparable.

The great master of our science, who, from the well-merited honours bestowed upon him by the Crown in recognition of his scientific services, has so long been known among us as Sir CHARLES LYELL, died on February 22nd, 1875. When, at our last anniversary meeting, I had the honour of addressing you, and while tracing the early history of our Society had occasion to refer to the services rendered to it during a period of upwards of 50 years by Sir Charles Lyell, little did I think that within three days' time we should have to deplore his loss, and that my first melancholy duty at the next anniversary would be to attempt some sketch of his most distinguished and meritorious career, and also to record our loss of his fellow Secretary of now upwards of fifty years ago, Mr. G. Poulett Scrope.

Charles Lyell was born on November 14th, 1797, at Kinnordy, the family estate in Forfarshire. His father, of the same name, was a man of both literary and scientific tastes, and of some distinction as a botanist—a remarkable genus of mosses, the species of which are principally found in the Himalayas, having received from Mr. Robert Brown the name of *Lyellia* in his honour. He was also well known as an Italian scholar, and published in 1835, and subsequently in

1842, some translations from Dante, including the poems of the Vita Nuova and the Convito, with various dissertations upon them. In 1845 he also published a translation of the lyrical poems of Dante. His death took place in 1849.

Mrs. Lyell was the daughter of Thomas Smith, Esq., of Maker Hall, Swaledale, Yorkshire. She died in 1850.

Their eldest son Charles, after receiving the rudiments of his education at two smaller schools, was sent to the school at Midhurst, under the head-mastership of Dr. Bayley. Even at the age of ten he had already exhibited a strong taste for natural history, especially for entomology, on which subject he found some books in his father's library. Not long after his son's birth, Mr. Lyell had removed to Bartley Lodge, near Lyndhurst, in the New Forest; and many were the hours spent by the young naturalist in watching the habits of insects and in forming a collection of them in so favourable a field. His interest in entomology continued throughout his school days, and indeed through all his life, remaining vivid even after his health had begun to fail. He had also already paid some attention to the study of geology, when, at the age of 17, he matriculated at Exeter College, Oxford. While at the University he appears to have taken the warmest interest in the lectures of Dr. Buckland, which no doubt exercised a strong influence over his future career. In 1818 he made a tour in France and Switzerland, in company with other members of his family, and thus commenced that long course of travel which is almost a necessity for those who wish fully to comprehend the grander features of geology and physical geography.

In the year 1819 he took the degree of B.A., obtaining a second class in classical honours—and that of M.A. in 1821; but in the former year, on February 19, 1819, he was proposed as an ordinary member of this Society. In his certificate, dated Feb. 16, he was described as of Exeter College, Oxford. It bears the well-known names of Wm. Buckland, W. D. Conybeare, and Wm. Edward Hony.

He was elected on March 19, 1819. On leaving Oxford, in that same year, he entered Lincoln's Inn, and commenced studying for the Bar. Owing, however, to the weakness of his eyes, he was recommended to desist for a time; and in 1820 he again travelled on the continent, in company with his father, on this occasion as far as Rome.

In December 1822 Mr. Lyell communicated to the Geological

Society his first paper, "A description of the sections presented by the banks of the rivers Isla, Melgum, Proson, and South Esk, in Forfarshire, with some remarks on the geology of that county, accompanied by specimens." This paper was not read until June 6, 1823, and does not appear to have been published, it having probably been withdrawn. In the meantime a joint paper by Gideon Mantell, Esq., and Charles Lyell, Esq., "On the beds of Limestone and Clay of the Iron-sand of Sussex," had been read on January 17, 1823. The action of the Society was in those days slower than at present; and it was not until March 1825 that the paper was ordered to be printed, in the 2nd series of the Transactions, vol. ii. p. 131.

On February 7, 1823, he was elected one of the Secretaries of the Society, in conjunction with Dr. Fitton and Mr. Webster, and retained the office until 1826, when he retired, after drawing up the Annual Report, but remained a member of the Council until 1827.

It was during this period of his Secretaryship that our Society was incorporated by Royal Charter.

About the year 1823 Mr. Lyell made more than one visit to Paris, where he formed the acquaintance of Baron Humboldt, Cuvier, Alexander Brongniart, Constant Prévost, and other distinguished men of science, whose friendship he retained in after life.

In 1824 he made another geological tour in Scotland, in the company of Dr. Buckland; and in December of that year he communicated to the Society another memoir, "On a recent formation of Rock-marl, or Freshwater Limestone, in the County of Forfar." This was followed by one in April 1825, "On Gyrogonites;" and in June he added some remarks on "Quadrupeds imbedded in recent alluvial strata," the main portions of which are incorporated with that on the Freshwater Limestone in the papers as printed in vol. ii. N. S. of the Transactions, p. 73 *et seqq.*

In May 1825 he contributed another paper, on a Dyke of Serpentine cutting through sandstone in the county of Forfar. This he withdrew, and published in the Edinburgh Journal of Science, vol. iii. p. 112.

In March 1826 we find some of the results of his residence in the New Forest, in his paper, "On the strata of the Plastic Clay Formation exhibited in the cliffs between Christchurch Head, Hampshire, and Studland Bay, Dorsetshire." In June this was followed by a memoir, "On the freshwater strata of Hordwell

Beacon and Barton Cliffs, Hants." Both of these are printed in the second volume of our Transactions, 2nd series.

In 1825 he was called to the Bar, and for two years he went the Western Circuit; but though few minds were more capable of sustained attention and the accurate grouping of facts and weighing of evidence, the pursuit of the law gave way to his devotion to science, and in 1827 he relinquished his legal career.

As will have been seen, he had for some years taken an active part in the affairs of our Society; and in 1827 I find it recorded in our Minutes that he seconded the proposal of Mr. Herschel, that the office of President or Vice-President should not be held for a longer period than two years—a practice which, previously to the granting of the Charter, had always prevailed in the Society, and which has since continued, with what are, I think, almost universally regarded as beneficial results.

In 1819 he became a Fellow of the Linnean Society.

In February 1826 he became a Fellow of the Royal Society, to the Philosophical Transactions of which he subsequently contributed more than one paper, and from which in later years he received both the Copley and Royal Medals in recognition of his distinguished services to science.

In June 1827 a paper by Mr. Lyell was read at a meeting of this Society, "On some fossil bones of the Elephant and other animals found near Salisbury." This memoir is of great interest, as recording the phenomena observed in the brick-earth and gravels at Fisherton Anger, a locality since that time again * brought into notice through the discovery of palæolithic implements in the same beds in which these bones of extinct animals were found. The author, with characteristic sagacity, remarked that in the brick-earth, which is a tranquil sedimentary deposit, there are no marine remains, but land-shells are said sometimes to occur, and that the brick-earth is not connected with the alluvial soil of the present valley, but appears to have been deposited when the valley was at a higher level, for it forms a low terrace along the side of the river Wiley, between Salisbury and Wilton, rising 30 or 40 feet above the present water-meadows. It is true that this is qualified by a remark that "it is necessary at least to suppose that when these beds were accumulated the water rose much higher than it now does;" but that Mr. Lyell had even at that time much the same views as those now generally held as to the erosive power

* See Q. J. G. S. 1864, p. 188.

of rivers, is evident from a paper communicated by him to the Society in December 1828, jointly with Sir Roderick (then Mr.) Murchison, "On the Excavation of Valleys, as illustrated by the Volcanic Rocks of Central France." In this paper they cite a valley 40 feet in depth, excavated in alluvial clay and sand, and partly in subjacent gneiss, by the waters of the Sioule, after the stream had been diverted from its course by the lava of Come—and another instance where the Sioule has cut through more than 100 feet of compact basalt, and also into the gneiss beneath, to a depth of at least 50 feet—and various other cases. They conclude that the elephant, rhinoceros, hippopotamus, hyæna, and other animals whose bones had then been recently disinterred from the sands and gravels, belonged to a period "before the most recent cones and lavas of the Auvergne had appeared, or the valleys had been excavated to their present depth."

The visit paid by Lyell and Murchison to the South of France in the summer of 1828 resulted in two other joint papers being communicated to this Society:—one in April 1829, "On the Tertiary Deposits of the Cantal and their relation to the Primary and Volcanic Rocks"*; the other in the following June, "On the Tertiary Freshwater Formations of Aix en Provence, including the Coalfield of Fuveau"†. It may be mentioned that in 1828 Mr. Lyell became a Vice-President of this Society, and in 1829 Foreign Secretary, an office which he held until 1835, when he became President.

From 1829 to 1833 he does not appear to have communicated any papers to the Society, his labours being concentrated on that great work on which, above all others, the fame of Lyell will rest, his 'Principles of Geology.'

Upon this work he had already been engaged for several years, as the original MS. was in the hands of the publisher towards the close of 1827, when it was proposed that it should appear in the course of the year following in two volumes octavo. It was while preparing the preliminary chapters on the History of Geology that he was seized with the desire of visiting several parts of the continent, in order more especially to acquire further information concerning the Tertiary formations. He was also anxious to verify the observations and views of his friend Mr. Poulett Scrope as detailed and exhibited in his 'Geology of Central France,' then recently published, and which had been reviewed by Mr. Lyell in

* Annales des Sci. Nat. xviii. 173. † Ed. New Phil. Journ. Oct. 1829.

the Quarterly Review for May 1827. Accordingly in May 1828 the tour in company with Murchison was undertaken, of which mention has already been made. They visited the Auvergne, Velay, Cantal, the Vivarais, and afterwards the environs of Aix en Provence, and then passed by the Maritime Alps to Savona, and thence across Piedmont. At Turin they found Signor Bonelli engaged in the arrangement of a large collection of Tertiary shells, principally from Italy; and here it would appear that Lyell first attempted the practical application of the idea he had already conceived of classing the different Tertiary groups by reference to the proportional number of recent species found fossil in each. After explöring some portions of the Vicentin, the travellers parted, and Lyell proceeded southwards, studying at Parma the fine collection of Tertiary fossil shells formed by Signor Guidotti, and visiting Florence, Sienna, and Rome. At Naples he formed the acquaintance of Signor O. G. Costa, who had studied the fossil shells of Otranto and Calabria, and had instituted some comparisons between them and the recent testacea of the Calabrian coast. In October 1828 Mr. Lyell examined Ischia, where he was surprised to find that of about 30 species of shells, some of them from beds 2000 feet above the sea, the whole, with but two or three exceptions, were of species now living in the Mediterranean.

From Naples he crossed into Sicily, where he spent about two months, and returned to Paris in February 1829. He there found M. Desnoyers just publishing his memoir on the Tertiary Formations more recent than the Paris basin, and communicated to him his views as to the means of arranging the Tertiary formations chronologically, M. Desnoyers being already convinced of there being a succession of Tertiary formations of different ages.

M. Deshayes had also arrived at the conclusion that the fossil shells of the Tertiary period might be arranged under three groups; and the views of the French conchologist and the English geologist proved to be so much in accordance, that the former promised the latter his co-operation in the great work which he had in hand, though Mr. Lyell was in favour of adopting four divisions instead of three.

In March 1829 he returned to London; but the printing of his book was a second time suspended while he again took the field in order to examine the Crag deposits of Essex, Suffolk, and Norfolk.

At length, in January 1830, the first volume of the 'Principles of Geology' made its appearance.

It is difficult to overrate the beneficial influence which this attempt to explain the former changes of the earth's surface by reference to causes now in operation has had on the progress of Geology. How many of us might re-echo the words of Professor Sedgwick spoken at the anniversary meeting of this Society in the year following the publication of the first volume of the 'Principles.' "Were I to tell the author of the instruction I received from every chapter of his work, and of the delight with which I rose from the perusal of the whole, I might seem to flatter rather than to speak the language of sober criticism, but I should only give utterance to my honest sentiments." In a subsequent passage Sedgwick suggests that in some portions of the work, as it at first appeared, the author might seem to forget the character of the historian in the language of the advocate. This may perhaps be regarded as another way of saying that, at the time, Sedgwick's own views and those of Lyell did not correspond. But even assuming that the assertion had some foundation in fact; which of us can say that, amidst the conflict of opinions, he can always hold the scales of even-handed justice, and never exalt one phase of truth somewhat at the expense of another, or that he can always take in at one glance the whole of the varied aspects of some single truth? In combating errors such as those with which Lyell had to contend, it was indeed impossible but that certain facts bearing directly upon points at issue should be insisted upon more strongly than those which were less important in the particular case, or even totally irrelevant. When arguments are otherwise unanswerable it is easy to meet them with a charge of partiality. Still, whatever may be thought upon this point, as a storehouse of facts, a mine of information, a model of logical argument, this portion of the 'Principles of Geology,' whichever may be the edition consulted, will ever stand almost unrivalled.

In the summer of 1830 Mr. Lyell, still intent upon his book, set out on a geological expedition to the South of France, the Pyrenees, and Catalonia, returning to Paris in September. In the following summer he made an excursion to the volcanic district of the Eifel; and on his return he determined to extend his work to three volumes. It was not until January 1832 that the second volume of the 'Principles' was published, when it was received with as much favour as the first had been. It related more especially to the changes in the organic world, while the former volume had treated mainly of the inorganic forces of nature.

Singularly enough, some of the points which were seized on by his great fellow-labourer Murchison in his Presidential Address to this Society in 1832, as subjects for felicitation, are precisely those which the candid mind of Lyell, ever ready to attach the full value to discoveries or arguments from time to time brought forward, even when in opposition to his own views, ultimately found reason to modify. We can never, I think, more highly appreciate Sir Charles Lyell's freshness of mind, his candour and love of truth, than when we contrast certain portions of the first edition of the 'Principles' with those which occupy the same place in the last, and trace the manner in which his judicial intellect was eventually led to conclusions diametrically opposed to those which he originally held. To those acquainted only with the later editions of the 'Principles' and with his 'Antiquity of Man' it may sound almost ironical in Murchison to have written "I cannot avoid noticing the clear and impartial manner in which the untenable parts of the dogmas concerning the alteration and transmutation of species and genera are refuted, and how satisfactorily the author confirms the great truth of the recent appearance of man upon our planet." It must, however, be remembered that it was with the theories of Lamarck, and not of Darwin, that the author of the 'Principles' had to contend; and, further, that in his later years no one was more ready to give Lamarck credit where credit was due, and especially with regard to his appreciation of Geological time.

Mr. Lyell had, in 1831, been appointed Professor of Geology in King's College, London, where, in 1832, he gave a course of lectures illustrative of the views explained in the concluding (and as yet unpublished) volume of his work.

In the summer of 1832 he made a tour up the valley of the Rhine, where he examined the loess, and visited the Valorsine on his way home through Switzerland, being still intent on the more recent geological formations.

At length, in May 1833, after the first and second volumes had already reached a second edition, the third and last volume of the 'Principles of Geology' was published, which was appropriately dedicated to his fellow-traveller Murchison.

Of its contents I need say but little beyond reminding you that the terms Eocene, Miocene, and Pliocene, now

"Familiar in our mouths as household words,"

were then first introduced into the English language. By the

work, as a whole, was dealt the most telling blow that had ever fallen on those to whom it appeared "more philosophical to speculate on the possibilities of the past than patiently to explore the realities of the present," while the earnest and careful endeavour to reconcile the former indications of change with the evidence of gradual mutations now in progress, or *which may be* in progress, received its greatest encouragement. The doctrines which Hutton and Playfair had held and taught assumed new and more vigorous life when their principles were explained by their eminent successor, and were supported by arguments which, as a whole, were incontrovertible.

But to return to Mr. Lyell's connexion with this Society. In February 1832 he communicated a paper, "On a Freshwater Formation containing Lignite, in Cerdagne, in the Pyrenees;" and in April 1833, "Observations on the Loamy Deposit called Loess, in the Valley of the Rhine."

In May 1834 he published a new edition, called the 3rd, of his 'Principles;' and during the summer of that year he made a tour in Denmark and Sweden, and subsequently communicated to the Royal Society his celebrated paper on the proofs of the gradual rising of the land in certain parts of Sweden.

On February 20, 1835, he was elected President of this Society, and in May of that year read a paper "On the Cretaceous and Tertiary Strata of the Danish Islands of Seeland and Möen," in which he gave some of the results of his visit to the islands in company with Dr. Forchhammer, dwelling mainly on the contorted chalk of Möen, and on the Faxoe beds. He also communicated a paper "On the occurrence of fossil vertebræ of fish of the Shark family in the loess of the Rhine, near Basle."

He continued in office until February 17, 1837, when he was succeeded by the Rev. William Whewell.

In August 1838 he published his second great work, 'The Elements of Geology,' which originally consisted of an expansion of the fourth book of the 'Principles.' This work also has gone through numerous editions, the sixth of which was published in 1865. In some of the intermediate editions it was termed a 'Manual of Elementary Geology;' but the work having outgrown the usual dimensions of a manual, its original title was resumed. As some measure of its growth, it may be mentioned that whereas the first edition consisted of 528 pages duodecimo, the sixth contained 772 pages octavo, of smaller and closer type. Of the great

merits of this text-book of our science I need hardly speak. To which of you are they unknown?

In 1839 he again joined the Council, and was one of the Vice-Presidents, and during that year communicated several papers to the Society. These were:—1. On the occurrence of Graptolites in the slate of Galloway in Scotland, the specimens having been found by Mr. J. Carrick Moore; 2. On some fossil and recent shells collected by Captain Bayfield, R.N., in Canada, the group bearing much resemblance to that of Uddevalla, with which Mr. Lyell compared it; and 3. On the relative ages of the Tertiary deposits commonly called Crag, in Norfolk and Suffolk, in which he substantiated the chronological arrangement of these beds, already suggested by Mr. Charlesworth.

In 1840 and succeeding years he still remained upon the Council, and in 1840 communicated papers "On the Boulder Formation or Drift, and associated freshwater deposits composing the mud cliffs of Eastern Norfolk" (a memoir published in the London and Edin. Phil. Mag. for May 1840), and "On the Geological evidence of the former existence of Glaciers in Forfarshire." In 1841 he read papers "On the freshwater fossil fishes of Mundesley, as determined by M. Agassiz," "On the Faluns of the Loire, and a comparison of their fossils with those of the newer Tertiary strata in the Cotentin, and on the relative age of the Faluns and Crag of Suffolk," "Some remarks on the Silurian strata between Aymestry and Wenlock," and "Notes on the Silurian strata in the neighbourhood of Christiania in Norway."

In 1841 Mr. Lyell was invited to deliver a course of twelve lectures at the Lowell Institution, in Boston, Massachusetts, and found his acceptance of the task repaid by an immense attendance at his class, for which 4500 tickets were issued, the average audience numbering about 3000. He took this opportunity of travelling through a considerable portion of North America—and in 1845 published an account of his travels, together with geological observations on the United States, Canada, and Nova Scotia, in two volumes. Though this work was essentially geological, the amount of general observations, not only on the institutions he found in America, but on those he left behind him in England, shows how warm an interest he took in all he saw, and with what true liberality of spirit his opinions were formed. Before the appearance of his 'Travels' he had communicated several papers on American geology to this Society:—the first in a letter from

Boston on the Carboniferous and older rocks of Pennsylvania; 2nd, a memoir on the recession of the Falls of Niagara; 3rd, on the Tertiary formations and their connexion with the Chalk in Virginia and other parts of the United States; 4th, on the fossil footprints of birds and impressions of rain-drops in the valley of the Connecticut; 5th, on the ridges, elevated beaches, inland cliffs, and boulder-formations of the Canadian lakes and valley of St. Lawrence; 6th, on the Tertiary strata of the island of Martha's Vineyard in Massachusetts; 7th, on the geological position of the *Mastodon giganteum* and associated fossil remains at Bigbone Lick, Kentucky, and other localities in the United States and Canada; 8th, on the upright fossil trees found at different levels in the Coal strata of Cumberland, Nova Scotia; 9th, on the Coal-formation of Nova Scotia, and on the age and relative position of the Gypsum and accompanying marine limestones; 10th, notes on the Cretaceous strata of New Jersey and other parts of the United States bordering the Atlantic; 11th, on the probable age and origin of a bed of Plumbago and Anthracite occurring in mica-schist near Worcester, Massachusetts; 12th, on the Miocene Tertiary strata of Maryland, Virginia, and North and South Carolina; and, 13th, observations on the White Limestone and other Eocene or older Tertiary formations of Virginia, South Carolina, and Georgia. Abstracts of most of these papers appeared in America, in Silliman's Journal.

The Cretaceous and Tertiary Corals collected by Mr. Lyell afforded also the foundation for several valuable papers communicated to the Society by Mr. Lonsdale.

In 1843 he revisited the Auvergne, and in November 1845 commenced a paper "On the age of the newest lava-current of Auvergne, with remarks on some Tertiary fossils of that country."

In the autumn of 1845 he returned, in company with his wife, to America, and furnished the Society with "Notices of the Coal-fields of Alabama and the newer deposits of the Southern States of North America," and papers "On the Footmarks discovered in the Coal-measures of Pennsylvania," and "On the structure and probable age of the James river, near Richmond, Virginia."

It was during this visit that he was enabled to make the important observations with regard to the formation of the alluvial plain of the Mississippi which he communicated to the British Association after his return to England in June 1846, and which are incorporated in the later editions of the 'Principles.'

In 1847 he again became a Vice-President of this Society, and read a paper "On the relative age and position of the so-called Nummulite Limestone of Alabama." He also made the reptilian footmarks he had observed in America the foundation of a lecture at the Royal Institution, an establishment in which he took a warm interest, and at which in subsequent years he gave several geological lectures of high value.

In February 1849 he contributed another American paper to this Society, in the shape of short notes on some recent foot-prints on Red Mud in Nova Scotia. Nor were these papers and letters the only result of these travels; for in 1849 he published "A second Visit to the United States of North America," in two volumes—a work containing, like the account of his former visit, a vast amount of geological observations, but in this case intermixed with a considerably greater proportion of general information and anecdote.

In 1848 he received the honour of knighthood; and in February 1849 he was again elected President of this Society, and remained in office until 1851, when he was succeeded by the late Mr. Hopkins. In December 1849 he communicated to this Society a most important paper, "On Craters of Denudation, with Observations on the structure and growth of Volcanic Cones;" and in 1851, one "On Fossil Rain-marks of the Recent Triassic and Carboniferous Periods," on which subject he also lectured at the Royal Institution. In May 1852 he again furnished a paper, "On the Tertiary Strata of Belgium and French Flanders;" and in the summer of that year he revisited America, as one of the Commissioners of the Great Exhibition in New York, and spent some time in Nova Scotia and New Brunswick. One of the results of this visit was a communication to the French Geological Society, "Rapport sur la partie géologique de l'Exposition de New York en 1853." Another was a joint paper by Sir Charles Lyell and Mr. J. W. Dawson, read to this Society in January 1853, "On the remains of a reptile (*Dendrerpeton acadianum*, Wyman and Owen), and of a land-shell discovered in the interior of an erect fossil tree in the Coal-measures of Nova Scotia."

A portion of the spring of 1854 was passed by Sir Charles Lyell in Madeira; and his letters to his father-in-law, Mr. Leonard Horner, whose eldest daughter he had married in 1832, afforded an interesting communication to this Society in March 1854, on the geology of some parts of Madeira.

From this time Sir Charles communicated no papers to this

Society. He remained, however, upon our Council until February 1865, taking an active part in our proceedings and constantly attending our meetings. In 1866, the fact of his having retired from the Council afforded the opportunity of awarding to him the Wollaston Medal; and never was there a more distinguished recipient. In 1867 he rejoined the Council, and remained one of its members until the day of his death.

His devotion to Geology, and to this Society as its chief exponent in this country, was such that he declined more than one possible career of utility in other departments, in order to remain free to attend our meetings, and to concentrate his energies upon his favourite science.

Nor can his interest in the welfare of our Society and of geological science be said to have ceased with his death; for in his will he bequeathed to the Society the dies for a medal, and a sum of £2000, the interest of which is to form a fund to be bestowed in the encouragement of geology, or any of the allied sciences by which geology may have been most materially advanced. The medal, as a mark of honorary distinction, will ever be highly valued; and the fund, under the wide and liberal conditions for its bestowal laid down by the testator, cannot be otherwise than productive of good.

Though his original communications to this Society had ceased in 1854, the pen of Sir Charles was never idle. One of the most important papers which he communicated to the Royal Society, that "On the structure of lavas which have consolidated on steep slopes, with remarks on the mode of the origin of Mount Etna and on the theory of Craters of Elevation," was read in 1858, and was subsequently translated into French, German, and Italian.

His principal occupation appears, however, to have been the accumulation of fresh stores of information, and recasting his 'Principles' and 'Elements of Geology,' of which a fifth edition appeared in 1855.

In 1859 his thoughts took a fresh direction; and the question of the antiquity of man, then brought prominently forward by the discoveries in the Brixham Cave, and by the confirmation of M. Boucher de Perthes's discoveries by the late Dr. Falconer and Mr. Prestwich, and, I may venture to add, myself, absorbed his immediate attention.

He had long taken part in the efforts of the British Association for the Advancement of Science, and, indeed, had been one of its

founders. He had presided over Section C. at Newcastle in 1838, again at Glasgow in 1840, at Birmingham in 1849, and at Aberdeen in 1859. It was at this latter place, to a crowded audience, among whom was the late Prince Consort, that Sir Charles, in his opening address, took the Antiquity of Man as his subject, and stated his belief that the age of the Amiens and Abbeville flint implements was great indeed if compared with the times of history or tradition. In February 1863 the first edition of his 'Antiquity of Man' was published; and so great was the demand for it that it reached a third edition by the following November. In it he not only summarized all the evidence in favour of a remote origin for the human race, but for the first time expressed his belief in Mr. Darwin's theory of the 'Origin of Species' as the best explanation yet offered of the connexion between man and those animals which have flourished successively on the earth; in neither case allowing preconceived opinions or a mistaken idea of consistency to interfere with his judgment or his impartial love of truth. In his fourth edition, which appeared in May 1873, much new matter is added, and the old in part modified; while the more recent discoveries, and his own observations during his travels in the south of France in 1872 are incorporated in the work.

In 1864 he was created a Baronet, and was also elected President of the British Association, which that year met at Bath, where, after dwelling in his address upon the problems connected with the origin of thermal springs and questions of climate, he again reverted to the antiquity of man, and related that now famous apologue as to the difficulty so often experienced when called upon to make liberal grants of time to what may be termed the recent period of geology, of "getting the chill of poverty out of our bones."

The 10th edition of the 'Principles of Geology,' which was greatly enlarged and filled two volumes instead of one as in the 9th edition, was published in 1866. In 1872 the 11th edition appeared; and during the last months of his life Sir Charles was busily engaged during all his best hours in preparing the 12th edition, which has been published since his death. Even on his deathbed he had the last few pages read to him, suggesting alterations and giving the necessary instructions to his secretary. It was but a few days before his death that he had the satisfaction of announcing to one of his friends "I have finished revising the 'Principles.'"

For some time before 1871 Sir Charles had strongly felt that he might aid in the great work of spreading a popular taste and interest in Geology by condensing his 'Elements' and bringing it out at a price which would just cover the cost of its preparation. The writing of his 'Student's Elements of Geology' was in consequence undertaken, and the first edition published in 1871, which was soon exhausted. The success of this attempt to popularize his favourite science induced Sir Charles—who often expressed his satisfaction at the manner in which this work of his old age had been received—to bring out a second and enlarged edition in 1874. Though the 'Student's Elements' was based on the earlier 'Elements of Geology,' the author regarded them, and with good reason, as two separate and distinct works.

As was so justly due to his eminent services in the advancement of knowledge, Sir Charles received numerous distinctions from academic bodies both at home and abroad. From the Universities of Oxford and Cambridge he received the honorary degrees of D.C.L. and LL.D. He was an honorary Fellow of the Royal Society of Edinburgh, a Knight of the Prussian Order of Merit, and a Corresponding Member of the French Institute (Academy of Sciences), of the Royal Academy of Sciences of Berlin, Munich, and Copenhagen, of the Philosophical Society of Philadelphia, of the Natural-History Society of Boston, and of other learned bodies.

In April 1873, Sir Charles suffered the saddest of all losses in the death of Lady Lyell, who for upwards of forty years had been to him an invaluable helpmate. To use the words of Principal Dawson, "his wife not only graced his home, and sedulously attended to all the wants and interests of a man too devoted to his specialties to give much attention to the ordinary affairs of life, but shared the fatigues of his journeys, and gave no small help in many of his works, being herself well acquainted with natural history and an accomplished linguist." From this loss he never fully recovered; and failing health and eyesight prevented him from often attending our meetings. Those, however, among us who had the good fortune to be present at the Jubilee dinner of the Geological-Society Club, in November 1874, will ever remember with pleasure the freshness and vigour of thought which he then exhibited, and which he preserved unto the end.

It was this freshness of mind which constituted one of his greatest charms; and the vivid interest with which he entered into every new subject and the attention which he concentrated upon

it were alike remarkable. I shall never forget the manner in which on one occasion he witnessed my manufacture of a flint arrow-head by means of a tool made of stag's horn—how he watched every splinter and scale as it was removed, and finally carried off not only the finished arrow-head, but every particle of flint which had been dislodged from the original flake in the process of manufacture. It was, too, this freshness and perpetual youth of mind which enabled him to take in and appreciate all new facts as they were discovered, to discard any of his former views which were proved to be erroneous, and to cheer on by his cordial sympathy the younger explorers in the field of science in which he had been so long a worker.

Many of us who have only known Sir Charles Lyell in his later years, have perhaps been too much accustomed to regard him rather as a systematizer of the facts and observations of others than as an original observer; and certainly no one was ever more preeminent in this most useful department of science, no one possessed greater intuitive knowledge of the value of any discovery, or was better able to see its relation to other facts, and to place it in its due position; and no one could do this in a more clear and attractive manner. It must, however, never be forgotten with how large an amount of travel and actual observation Sir Charles Lyell began his course, as is fully evinced by his numerous communications to this Society, nor that practically the progress of science has been much further advanced by those who, after fully qualifying themselves for the task, have, like Sir Charles Lyell, brought themselves into close contact with their fellow-workers in the same field, and reduced their discoveries into one harmonious whole, than would have been the case had the same persons confined themselves to original work in what after all could be but a small area as compared with the whole expanse of their science.

With regard to his original work, I am tempted to use the words of a letter written by Mr. Leonard Horner at least 35 years ago. "Lyell's reputation rests on his having, in his 'Principles,' put forth grand generalizations of the science of Geology in a masterly manner. He was the first to show the changes of climate which would necessarily follow the existence or absence of land above the surface of the sea, and its varied elevation, and that such changes in the relative proportions of sea and land must have been going on at various epochs all over the globe. He has also propounded new views in regard to the successive ages of the more modern parts of

the earth's surface—the Tertiary Strata—by showing that their relative ages may be determined by the proportions of the remains of living organisms imbedded in them."

There is one other point on which it seems desirable to say a few words; and inasmuch as I find, on reading Principal Dawson's address to the Natural-History Society of Montreal, that he has anticipated me on the subject, I make no apology for using his language rather than my own. He says that injustice has been done to Lyell by a misconception that he was thoroughly uniformitarian in the sense of maintaining that similar changes have been taking place throughout all geological time. "It is true that he objected to any explanation of geological changes by imaginary cataclysms not warranted by observations of similar facts; but no one was more ready than he to receive any evidence of change, or physical or organic action, whether sudden or gradual, as a geological cause, provided it could be shown to be or to have been a natural fact. Further, no one was more fully impressed with the continual change and progress in nature, and with the necessity of taking into account the different conditions of different geological times, in applying any modern cause to account for ancient phenomena."

It is, however, time to conclude this imperfect memoir, which has extended to an almost unprecedented length, but which I fear has not done justice to one whose memory we all so highly honour, and to whose labours the whole of the civilized world is so deeply indebted. His death, which was somewhat accelerated by an accident, took place on February 22, 1875. The remains of Sir Charles Lyell received a fitting tribute of honour by being interred among others of our good and great in Westminster Abbey. In the words of the solemn anthem which we heard over his grave, "His body is buried in peace, but his name liveth evermore."

Sir Charles Lyell's early and constant friend, Mr. George Poulett Scrope*, F.R.S., did not long survive him. He was born in London in 1797—the year in which also Sir Charles was born—and was the second son of J. Poulett Thomson, Esq., of Waverley Abbey, Surrey, the head of the eminent mercantile firm of Thomson, Bonar and Co. He was educated at Harrow, and subsequently proceeded to

* For portions of this memoir I am indebted to a biography of Mr. Scrope, revised, I believe, if not written by himself, in the Geological Magazine for May 1870.

St. John's College, Cambridge. While still an undergraduate, he passed the winter of 1816–17 at Naples, with a part of his family, where he was struck by the phenomena of the neighbouring volcano, then in almost permanent though moderate activity. Returning to Naples in 1818, he renewed his study of Vesuvius, and the volcanic territory of the Campagna, and of the district west of the Apennines, between Santa Fiora, in Tuscany, and the Bay of Naples, in which, however, no eruption has taken place within the last few centuries.

At Cambridge he had the advantage of frequent intercourse with Professor E. D. Clarke and Professor Sedgwick, who was then commencing his distinguished career as a geologist. They both agreed with him in thinking that the influence of volcanic forces in the production of the rocks that compose the surface of the globe had been much undervalued, and encouraged him to continue his researches.

In the spring of 1819 he therefore made the tour of Sicily, visiting Etna and the Lipari Islands. Being still more strongly convinced of the erroneous nature of the Wernerian views, which were then in vogue, as to the origin of the so-called Flœtz Trap-rocks, he determined to proceed to Auvergne.

On his marriage, in 1821, with the heiress of the ancient family of Scrope, he assumed that name; and in June of that year Mr. Scrope established himself at Clermont, the capital of the Department of the Puy de Dôme, and passed some months in continual examination of the geology of the neighbourhood, removing thence, as it became convenient, to the Baths of Mont Dore, Le Puy, and Aubenas. He there collected the materials for the volume on the Geology of Central France, published by him some years later (in 1827), a work which has been ever since generally accepted as the best authority on this interesting district.

From France Mr. Poulett Scrope proceeded to Italy, where, after visiting the Euganean Hills and other volcanic districts of Northern Italy and of the Roman territory, he reached Naples once more, in the beginning of October, 1822, fortunately just in time to witness the great paroxysmal eruption of that month, which left Vesuvius lowered in height by some 600 feet, and transformed it from a solid cone, with a nearly flat rough plain at the summit, into a hollow crust or casing, as it were, still outwardly cone-shaped, but pierced internally by a vast crater, a mile in diameter and nearly 2000 feet deep, which had been torn through the heart of the moun-

tain by powerful continuous explosions of twenty days' duration. The study of this stupendous example of volcanic energy, and of its effects, impressed Mr. Poulett Scrope with those opinions on the true character and mode of action of volcanic force which distinguish his views on this subject from those of the greater number of writers who have treated upon the same matters, but who possessed a less fortunate combination of opportunities for the direct personal observation of the phenomena and the formation of a sound judgment as to their real nature.

Mr. Scrope contributed to the 'Journal of Science' (vol. xv. p. 175) a short account of the great eruption of 1822, with a drawing of the crater as it appeared immediately afterwards.

In the summer of 1823 he spent some time in the examination of the volcanic region of the Upper and Lower Eifel, the Siebengebirge, &c., and subsequently sent a detailed description of them to the 'Edinburgh Journal of Science' (June 1826). Returning to England, he was proposed as a Fellow of this Society, and was elected on April 23, 1824, Sir Charles Lyell's name being among those appended to his certificate. At the time of his election he communicated a paper to the Society "On the Geology of the Ponza Islands," which afforded matter for reading at three successive meetings. It is printed in our Transactions. He had already, in 1823, published some articles in the 'Quarterly Journal of Science,' "On the Geology of the Paduan, Vicentine, and Veronese territories," and appears also to have written on the fossil fishes of Monte Bolca. In February 1825 he was elected one of the Secretaries of this Society in conjunction with Sir Charles Lyell, but retired from the office at the end of the year, remaining on the Council until 1827.

In March of the latter year he communicated a paper "On the Volcanic district of Naples."

In 1825 he published the first edition of his work 'On Volcanos, the character and probable causes of their Phenomena, and their connexion with the present state and past History of the Earth,' a work which, owing to its strong opposition to the Wernerian doctrines as to the aqueous precipitation of Trap-rocks, which were at that time in the ascendant, was received by many with distrust, and by some even with ridicule. A few years later, however, the justice of Mr. Poulett Scrope's views as to the volcanic origin of Basalts, Trachytes, Porphyries, and similar rocks, and of his ideas as to subterranean agency began to be acknowledged; and at the end of 1826, when his volume 'On the Geology and Extinct Volcanos

of Central France' issued from the press, illustrated by elaborate sketches made upon the spot, a different opinion began to be formed as to the authority of his earlier work.

One feature of great interest in the case is that in the 'Quarterly Review' for May 1827 appeared an article "On the Geology of Central France," which, in the words of Mr. Scrope, was, "I believe, the first essay of my distinguished friend Sir Charles Lyell in the path of geological generalization which he has since so successfully pursued."

There can be no doubt as to the community of ideas which existed between Scrope and his fellow Secretary, which, indeed, formed a bond of intimacy and of mutual regard between them. Both the one and the other were the staunch advocates of the doctrines of Hutton and Playfair as contrasted with those of Werner; and there cannot well arise a question of priority between them. The following passages, extracted from the "Preface to the Considerations on Volcanos," published in 1825, might almost have been written by the author of the 'Principles of Geology,' and are well worthy of being reproduced:—

"Geology has for its business a knowledge of the physical processes which are in continual or occasional operation within the limits of our planet, and the application of these laws to explain the appearances discovered in our geognostical researches, so as from these materials to deduce conclusions upon its past history.

"The surface of the globe exposes to the eye of the geognost abundant evidence of a variety of changes which appear to have succeeded one another during an incalculable lapse of time. These changes are briefly:—

"1. Variations of level between different constituent parts of the surface of the terraqueous globe.

"2. The destruction of former rocks and their reproduction under new forms.

"3. The production of new rocks upon the earth's surface. Geologists have hitherto usually had recourse for the explanation of these changes to the supposition of sundry violent and extraordinary catastrophes, cataclysms, or general revolutions.

"As the idea imparted by the terms cataclysm, catastrophe, or revolution is extremely vague, and may comprehend any thing you choose to imagine, it answers for the time as an explanation; that is, it stops further inquiry; but it has the disadvantage of stop-

ping also the advance of the science by involving it in obscurity and confusion.

"If, however, instead of forming guesses as to what may have been the possible causes and nature of these changes, we pursue that which I conceive to be *the only legitimate path of geological inquiry*, and begin by examining the laws of nature which are actually in force, we cannot but perceive that numerous physical phenomena are going on at this moment on the surface of the globe by which various changes are produced in its constitution and external character."

He then considers the nature of these changes and their analogy with those which must have occurred in earlier ages of the world's history, and adds, "until after a close investigation and the most liberal allowance for all possible variations, and *an unlimited series of ages*, they have been found wholly inadequate to the purpose, it would be unphilosophical to have recourse to any gratuitous and unexampled hypotheses for the solution of these analogous facts."

But to return to the other writings of Mr. Scrope. In 1826 he communicated to the Edinburgh Journal of Science some "Observations on Humboldt's Theory of the Volcano of Jorullo," and "On the volcanic formation of the left bank of the Rhine," and to the 'Quarterly Journal of Science' a "Descriptive arrangement of Volcanic Rocks."

In February 1830 his paper "On the gradual excavation of the valleys in which the Meuse, the Moselle, and some other rivers flow" was read to this Society. This was following up his views as to the excavation of the valleys of the Auvergne. Lyell and Murchison had, as I have already stated, followed Scrope's observations in that district, and confirmed their accuracy; and it has been * cited as a remarkable instance of the convincing character of Scrope's work, that both the one and the other, though holding the most divergent opinions on all similar questions—equally maintained in their latest writings the accuracy of his conclusions in the Auvergne district.

In 1831 Mr. Poulett Scope, who had for some years been settled in the ancient family seat of the Scropes, of Wiltshire, Castle Combe, furnished this Society with a paper "On the rippled markings of many of the Forest-marble beds north of Bath, and the foot-tracks of certain animals occurring in great abundance on

* See Memoir of Mr. Poulett Scrope, by J. W. Judd, F.G.S., in the 'Academy,' January 29, 1876.

their surfaces," which was destined for some time to remain the last of his geological essays.

For already, in 1830, he had begun to neglect Geology for what he regarded as the more practical subjects of political and social economy. His opinions on the Poor-law, the Currency, Banking, and other questions were made public in a long series of pamphlets, reviews, and other contributions to the periodical literature of the day. His brother, Mr. Poulett Thomson, afterwards Lord Sydenham, had been for some years in Parliament; and after the passing of the Reform Bill, Mr. Scrope was in 1833 returned as one of the members of the newly constituted borough of Stroud, which place he represented for 35 years, until his retirement in 1868. Though devoting himself mainly to political questions, he still occasionally found time for geological and antiquarian work. In 1835 he reviewed in the 'Quarterly' the third edition of his friend Lyell's 'Principles of Geology,' expressing his admiration for the author and agreement with him on almost all points, except the too vigorous application of the doctrine of uniformity in the series of geological changes. He was also more ready at that time than Lyell to accept the idea of progressive development with regard to organic forms.

In 1856 he communicated to this Society a paper "On the formation of Craters and the nature of the liquidity of Lavas," in which he combated the famous Elevation-Crater theory of Humboldt, Von Buch, and Élie de Beaumont.

In 1857 he revisited Auvergne, in anticipation of the second edition of his 'Geology and Extinct Volcanos of Central France,' which was published in 1858. In 1859 he returned to the charge against the Elevation-Crater theory, then lately again brought into prominence by the publication of Humboldt's 'Kosmos,' and in another paper on the mode of formation of volcanic cones and craters confirmed the views of Sir Charles Lyell as displayed in his celebrated communication to the Royal Society, on the lavas of Mount Etna. It would appear as if in consequence of these joint efforts the unphilosophical character of the theory they attacked had since that time been universally acknowledged.

Earlier in 1859 he had communicated to this Society a paper "On the Lamination and Cleavage occasioned by the mutual friction of particles of rocks while in irregular motion," which he subsequently withdrew.

In 1862 he superintended a new edition of his work 'On

Volcanos,' with an enlarged description of all the known volcanos and volcanic formations of the globe. Since that time he has been a frequent contributor of papers of great interest to the pages of the 'Geologist' and its successor, the 'Geological Magazine.'

In 1867 the richly deserved honour of the Wollaston Medal was awarded to him by the Council of this Society, in recognition of the highly important services he had rendered to Geology by his examination and published descriptions of the volcanic phenomena of Central France, and by his works on the subject of volcanic action generally throughout the world.

Of his other writings may be mentioned the 'History of the Manor and Ancient Barony of Castle Combe, in the County of Wilts,' which was printed for private circulation in 1852. It is a handsome 4to volume, with numerous illustrations, and full of historical research. Portions of the work had already appeared in the 'Wiltshire Magazine,' to which he was a frequent contributor —having been for many years President of the Archæological and Natural-History Society of that county. Besides antiquarian papers, he furnished that Magazine with a popular essay "On the Geology of Wiltshire," and one "On the origin of the Terraces, Bulks, or Lynchets of the Chalk Downs," in which he combated the "sea-beach" origin ascribed to them by Mr. D. Mackintosh.

After relinquishing his seat in Parliament in 1868, he lived in complete retirement; but, though suffering from defective sight, he kept pace with the geological progress of the day, having in his old age found a solace in returning to the favourite study of his youth. Even within a few weeks of his death, a friendly controversy in the pages of 'Nature,' between him and the President of the Royal Society, showed how unimpaired was still the activity of his mind, and how vivid was his remembrance of the scene of his early explorations. His kindly manner and courtesy endeared him to all who were brought into contact with him; and more than one of his surviving friends can testify to his readiness to assist them with more than mere words, and to the friendly and large-hearted liberality with which he stimulated and assisted younger labourers in his own domain.

He died at Fairlawn, near Cobham, in Surrey, on the 19th of January last; and his remains are interred in the quiet country churchyard of Stoke d'Abernon.

Next in succession to Sir Charles Lyell and Mr. Poulett Scrope

must be mentioned another veteran, who was almost their contemporary, Sir William Edmond Logan, F.R.S.*

He was born in Montreal in 1798, of Scottish parents, and after commencing his education in Mr. Shakel's school, in that city, completed it at the High School and University of Edinburgh. On leaving college he entered the counting-house of his uncle, Mr. Hart Logan, in London, where he remained about ten years, and was even at that time attached to the study of geology.

In 1829 he paid a visit to Canada; but returning the same year to this country, he settled at Swansea, and in partnership with Mr. Starling Benson he undertook the charge of some copper-smelting works and a colliery in which one of his uncles was interested. In 1834 he made a tour through France and Spain, visiting many of the mines in the latter country, and examining the fossiliferous beds of Touraine. While at Swansea he was an active promoter of the Royal Institution of South Wales, having been associated with our present Treasurer, Mr. J. Gwyn Jeffreys, as one of the Honorary Secretaries and the Curator of the geological department. He also collected a vast amount of geological information with regard to the South-Wales Coalfield, which he generously placed at the disposal of Sir Henry De la Beche, when he began the Geological Survey of the district. So excellent was his work that it was adopted by the Government Survey; and the name of Logan appears on the early maps of the district, in conjunction with those of De la Beche, Ramsay, Phillips, and Aveline. In 1837 he was elected a Fellow of this Society; and in 1838, on the death of his uncle, he resigned his position at Swansea, and devoted himself more completely to geological pursuits. His first geological paper was "On that part of the South Welsh Coal Basin, which lies between the Vale of Neath and Carmarthen Bay," which was communicated to the British Association in 1837. In 1840 his important paper "On the character of the beds of Clay lying immediately below the Coal-seams of South Wales, and on the occurrence of Coal-boulders in the Pennant Grit of that district," was read to this Society. In it he pointed out, for the first time, how each coal-seam rests on an underclay, with rootlets of *Stigmaria* in it, and suggested that this was the soil in which the *Stigmaria* grew, and that

* For many of the particulars in this memoir I am indebted to a paper by Dr. B. J. Harrington, communicated to the Nat.-Hist. Soc. of Montreal, and to a notice by Prof. A. Geikie, F.R.S., in 'Nature,' July 1, 1875.

the coal now stood in the place of the matted vegetation which grew upon that soil. It is needless to point out the value of this happy deduction.

In order more fully to verify its general application, he visited America in 1841, and examined the coalfields of Pennsylvania and Nova Scotia, recording his observations in a paper published in our Proceedings, which also contains his remarks on the packing of ice in the river St. Lawrence, and on some geological phenomena he had noticed in the neighbourhood of Montreal.

Whether it was in any degree owing to Mr. Poulett Scrope's brother (Lord Sydenham) having been Governor-General of Canada up to 1841, that a desire arose in the colony for the institution of a Geological Survey, it is now perhaps impossible to say. But early in 1842 a sum of £1500 having been voted for the purpose, the appointment of a Provincial Geologist was referred to the late Earl of Derby, who, on the recommendation of Murchison, De la Beche, Sedgwick, and Buckland, appointed Mr. Logan, whose name had already been mentioned by the Canadian authorities.

In August, 1842, he arrived in Canada, and at his own expense spent several months in making a preliminary examination of the country, but returned to England to make his final arrangements. He was fortunate enough to secure the aid of Mr. Alexander Murray, who is now directing the Geological Survey of Newfoundland.

He commenced his actual work in May 1843, his first business being to examine portions of the Coalfield of Nova Scotia and New Brunswick. It was at this time that he made his celebrated Section of the Coal-measures of the South Joggins, which gives details of nearly the whole thickness of the coal formation of Nova Scotia, or 14,570 feet, including seventy-six beds of coal and ninety distinct *Stigmaria*-underclays. The summer and autumn of the same year he spent in examining the coast of the Gaspé Peninsula in order to determine whether any outlying patches of the Coal-measnres were there to be found; and the following summer he devoted to the Copper-bearing rocks of Lake Superior, where he discovered the same old chloritic slates which he had, in 1845, observed on Lake Temiscaming, and to which the name of Huronian has since been given.

In subsequent years he studied the eastern parts of Canada, and in 1850 the gold-bearing drift of the Chaudière, but devoted much time to the collection of the specimens which attracted so great an

amount of attention in the London Exhibition of 1851. He spent part of that year in England, rejoicing to meet his old geological friends; and the opportunity was taken of electing him a Fellow of the Royal Society, from which body he received one of the Royal Medals in 1867.

He returned to Canada in August 1851, and continued the examination of the country on the north side of the St. Lawrence during the following years, and, in his Report of 1854, first designated the rocks as Laurentian which had hitherto been only known as the Metamorphic Series or Fundamental Gneiss.

He eventually showed how these rocks are older than the Huronian, and that those two unconformable groups attain together to a thickness of not less than 30,000 feet. He was among the first to recognize the organic character of the *Eozoon canadense* from this formation, and exhibited it as a fossil in 1857, some years before its true structure and affinities were determined by Dawson and Carpenter.

In 1854 and 1855 his time was much taken up with preparations for the Paris Exhibition, which he attended as one of the Canadian Commissioners. At Paris he was made a Chevalier of the Legion of Honour; and early in 1856 he received the honour of knighthood in this country. It was in that year that the Council of this Society awarded to him the Wollaston Medal, for his elaborate papers on the origin and structure of the Coal-beds in England, and for his subsequent labours in Canada in carrying out the Geological Survey of that country, and particularly for his admirable Geological Map of Canada.

On his return to Montreal the citizens presented him with a testimonial, and the Natural-History Society with an address, while the Members of the Canadian Institute of Toronto, of which Sir William Logan was the first President, had his portrait painted and hung up in their hall.

In 1857, after the meeting of the American Association for the Advancement of Science at Montreal, at which he read papers "On the Huronian and Laurentian Series of Canada" and "On the subdivision of the Laurentian Rocks in Canada," he made a geological tour through the State of New York in company with Professor Ramsay. His subsequent work, up to 1869 when he resigned his appointment to Mr. A. R. C. Selwyn, our Murchison Medallist of this day, was mainly the preparation and publication of the 'Geology of Canada,' with its accompanying Atlas.

He found time, however, for work in the field, especially among the Laurentian rocks of Grenville and the Pictou Coalfield.

The few remaining years of his life were occupied chiefly with a study of the rocks of the eastern townships of Canada, and of part of New England; but the results of this portion of his labours have not been published.

His difficulties in the conduct of the Geological Survey of Canada, especially during the first years of its existence, were enormous; and nothing but the almost superhuman energy and industry which he exhibited in preparing his reports, and his tact in management, and great power of organization, would have carried him through to a successful result. By the strictest economy, by judicious choice of assistants, by the enlistment of volunteers in his service, and by directing his observations to points where the mineral resources of the colony were most likely to be rendered available, he converted an institution which had been looked on with but lukewarm interest at best, into one which is now among the most popular in the colony. In the words of the 'Daily Globe' of Toronto, in an article upon his retirement, "It may be safely said that from no other department of the administration have such results been obtained as from the Geological Survey.... Many times have Sir William Logan's statements, in reference to economical minerals, been questioned, sometimes even with what appeared at the moment to be justice. Experience, however, invariably proved him to be right. He has always wisely and prudently guarded his statements, so as to prevent reckless expenditure of money in unproductive mines, while he has given all needful encouragement to reasonable and intelligent hopes of a financial return. It is something for him to say in leaving the Survey, that his statements have deceived no one, that not one dollar has been expended by reason of errors or too sanguine statements on his part."

Besides the papers already mentioned, he communicated to this Society, in 1852, one on the "Footprints occurring in the Potsdam Sandstone of Canada." Sir William also published several memoirs, in the 'Canadian Journal,' the 'Canadian Naturalist,' and 'Silliman's Journal;' but his reports alone would fill several volumes. Personally he was a man of frank simple manners, of great liberality, and earnestly devoted to his profession. From his attachment to his native country of Canada, he declined the great inducements offered to him by the East-India Company, in 1845, to proceed to India with the view of investigating its resources in

coal. It was the same liberal and patriotic feeling which led him to advance considerable sums of money from his private resources, in order that the operations of the Survey might not be interrupted in its early years. In 1864 he endowed the McGill University with the 'Logan Gold Medal' for an honour course in Geology and Natural Science; and in 1871 he endowed the Logan Chair of Geology of that University with $19,000, to which his brother, Mr. Hart Logan, added another $1000. Sir William, who, like his four brothers, never married, died at Castle Malgwyn, Llechryd, South Wales, on the 22nd of June last.

Mons. Gérard Paul Deshayes was born May 24th, 1796, at Nancy, where his father was Professor of Experimental Physics in the École Centrale of the Department of la Meurthe. After studying in the Faculty of Medicine at Strasburg, where he obtained the first prize for anatomical dissection, he came to Paris about the year 1820, and took the degree of "Bachelier ès lettres" in 1821. Abandoning the medical profession, he devoted himself to natural history, and especially to geology and conchology. His first memoir on any geological subject was published in 1823, its subject being the fossil shells of Valmondois, between Pontoise and l'Isle Adam, where he had discovered several new and interesting species. He had already devoted much attention to the fossil shells of the Paris Basin, and in 1824 commenced, at his own risk and charges, his great work, the 'Description des coquilles fossiles des environs de Paris.' This work, for a time interrupted, owing to misfortunes in his family and pecuniary difficulties, was brought to a conclusion in 1837. After twenty years of further study he returned to the subject in 1857, when he published the first part of his 'Description des Animaux sans vertèbres découverts dans le bassin de Paris,' which was completed in 1867. This study of the fossil shells of the Paris Basin, their connexion with the strata in which they occur, and their sequence in time may, indeed, be regarded as the great work of his life. He had already arrived at the conclusion that the fossil shells of the Tertiary Period might be grouped under three main divisions, when, as I have already stated, he was brought into contact with Sir Charles Lyell, whose investigations had led him to much the same result. Mons. Deshayes rendered Sir Charles most valuable assistance by furnishing him with the Statistical Tables which appeared in the 'Principles of Geology.'

In 1833 he undertook the description of the Mollusca, both

recent and fossil, brought home by the scientific expedition to the Morea, and in 1834 performed the same task for the collection of shells formed in India by M. Bélanger. In 1839 he began the publication of his 'Traité élémentaire de Conchyliologie, avec l'application de cette science à la géognosie,' which, however, was not finished until 1857. It was, indeed, in 1839 that he was selected by the Academy to take part in the labours of the scientific commission which was sent by the French government to Algeria. He remained in that country until 1842, not only collecting but making drawings of his specimens. The publication of his 'Mollusques de l'Algérie' was unfortunately interrupted by the Revolution of 1848, after 150 coloured plates and 600 pages of text had appeared. Jointly with M. Milne-Edwards, M. Deshayes revised the 'Histoire naturelle des animaux sans vertèbres' of Lamarck. He also assisted in the publication of the 'Histoire naturelle des Mollusques' of Férussac.

Soon after his arrival in Paris he commenced giving private lectures on geology, with occasional excursions in the field. Among those who attended his classes may be mentioned Messrs. E. de Beaumont, D'Archiac, de Verneuil, Constant Prévost, Desnoyers, and Hébert.

He was for many years a distinguished member of the Geological Society of France, of which he was one of the founders, and more than once the President; and in 1869 he was nominated Professor of Conchology at the Muséum d'Histoire Naturelle at Paris. His fine private collection was purchased by the French Government in 1868, and deposited in the École des Mines.

Though the greater part of M. Deshayes's work was confined to France, the article "Conchifera" in Todd's Cyclopædia of Anatomy and Physiology is attributed to his pen; and he also commenced a catalogue of the bivalve shells in the British Museum, of which only the first part, relating to the Veneridæ, was published.

In 1836, under the presidency of Sir Charles Lyell, he received from our Society the proceeds of the Wollaston Fund to assist him in his labours on fossil conchology, and in 1856 and again in 1864 this same fund was awarded to him in testimony of the high value which this Society had always attributed to his labours, and in the hope that it might be of some use to him in the further prosecution of his important work on the Mollusca of the Paris Basin. In 1870, just 50 years after his arrival in Paris, the Wollaston medal was awarded to him as the highest honour the Council of this Society

could bestow upon the pupil, editor, and continuator of Lamarck, and the worthy successor of his great master in the chair of Natural History in the Muséum d'Histoire Naturelle.

In 1841 he was elected a Foreign Member of this Society. A translated abstract of one of his papers "on the fossil shells of the Pyrenees" will be found in our Journal; and several of his papers are printed in the Proceedings of the Zoological Society. His other memoirs it is needless to cite. A list of upwards of 70 will be found in the Royal Society's Catalogue.

In the winter of 1873 failing health induced him to quit Paris for the South of France; but in 1874 he still occupied himself with his duties at the Museum, and with another great work, the 'Nomenclator Malacozoologicus,' which he was not destined to see published. In the spring, however, of 1875 he retired to Boran, near the spot where he had collected the specimens on which his first memoir had been founded upwards of 50 years previously; and there he peaceably expired on the 9th June 1875, after 79 years of a life devoted to science.

Another name which we must this year include in our obituary is that of Mr. William Jory Henwood, F.R.S., to whom, at our last Anniversary Meeting, the Murchison Medal was presented. It will be remembered that, in consequence of ill health, he was unable to attend personally to receive it, and that it was conveyed to him through our Secretary, Mr. David Forbes. I have good grounds for believing that this recognition of the services of Mr. Henwood in the cause of Geology and Mineralogy, extending over a period of fifty years of laborious research, was a source of great satisfaction to him, and afforded him some solace in the midst of much physical suffering.

He was born* at Perron Wharf, Cornwall, on the 16th of January, 1805, and was the son of Mr. John Henwood, a member of an old Cornish family, settled at Levalsa, in St. Ewe. His grandfather appears also to have taken an interest in mining matters, but unfortunately one that entailed considerable loss, as he became connected with the first Cornish silver-mine, the "Huel Mexico," from which, though £2000 worth of ore was raised, yet the cost of raising it far exceeded its value.

His father was for several years a clerk in the service of Messrs. Fox and Co., of Perron Wharf; and it was in their office that the

* For most of this memoir I am indebted to Mr. W. Prideaux Courtney.

son began life, in 1822. Here he remained about five years. During this period the nature of his employment happily enabled him to commence those investigations into the metalliferous deposits of Cornwall and Devon which for the next fifty years were destined to occupy so much of his attention. His first underground visit to a mine was to the Wheal Herland, in Gwinear, in 1825; and in the following year he communicated to the Royal Geological Society of Cornwall his first paper, being one "On a singular Extrication of Gas in the Union Mines." For the next thirty years his pen was constantly active, a list of no less than fifty-five of his papers appearing in the Royal Society's Catalogue, and a still longer list in the 'Bibliotheca Cornubiensis.'

In 1828 he was elected a Fellow of this Society, and he printed at Truro some "Observations on the performance of a Steam-engine recently erected at Huel Towan," which was reprinted, though anonymously, in the 'Edinburgh Journal of Science,' to which, as well as to other scientific periodicals, he was subsequently a frequent contributor.

In 1832 Mr. Henwood was appointed to the office of Assay-master and Supervisor of Tin in the Duchy of Cornwall, an office which he held until the coinage-duties were abolished in 1838, when he retired on a pension.

In 1832, he furnished this Society with notes "On some intersections of mineral veins in Cornwall." In 1837 he communicated to the Institution of Civil Engineers a paper "On the Expansive Action of Steam in some of the pumping-engines in the Cornish Mines," for which he was awarded a Telford Medal. In February 1840 he was elected a Fellow of the Royal Society, to which he communicated some experiments on the electric conditions of the rocks and metalliferous veins (lodes) of Longclose and Rosewall-Hill Mines in Cornwall.

In 1841 he communicated to this Society "A brief note to accompany a series of specimens from Lockport, near Niagara, in the State of New York," and "Notes to accompany a series of specimens from Chaleur Bay and the river Ristigouche, in New Brunswick."

In 1843 he left this country in order to take charge of the Gongo-Soco Mines in Brazil, where he devoted much attention to the amelioration of the condition of the slaves by whom the mines were worked. Some account of the means which he adopted was published by him at Penzance in 1864.

From Brazil he proceeded to India, where in 1855 he drew up for the Indian Government a report on the metalliferous deposits of Kumaon and Gurhwal, which was published in the 'Indian Government Records,' and in the 'Edinburgh New Philosophical Journal." A second report on some iron formations in Bengal still slumbers in manuscript.

In 1858, with impaired health, he retired from active labour, and spent the remainder of his life among his friends at Penzance.

His most important memoirs, "On the Metalliferous Deposits of Cornwall and Devon, with observations on subterranean temperature," "On the quantities of water which enter the mines," and "On the electric currents observed in the rocks and veins," together with a large amount of statistical observations, had been published by the Royal Geological Society of Cornwall in 1843, and had constituted the fifth volume of its transactions.

The history of how this work was completed is highly creditable to all concerned. Mr. Henwood had hardly entered the service of Messrs. Fox before he commenced the study of the metalliferous deposits of Cornwall and Devon; and for some time he was compelled to restrict his investigations to a limited district. In 1828, however, his employers enabled him to prosecute his researches over a larger area and with greater activity. As his own pecuniary resources were not sufficient, the gentry of the county subscribed a sum of about £200 to aid Mr. Henwood in carrying out his self-imposed task, while the Society undertook the publication of the results of his valuable labours.

Before leaving this country his researches had been mainly confined to Cornwall; but by his visit to Brazil and India Mr. Henwood had been enabled to prosecute his studies over a more extensive field of observation; and on his return to England in 1858, his mind was enriched with information which he had acquired in every part of the world devoted to mining operations. His second volume on metalliferous deposits, published, like its predecessor, by the Royal Geological Society of Cornwall, was so far widened as to comprehend observations on iron, copper, silver, and other metals discovered in the East and West Indies, North and South America, and the whole of Europe. The publication of these volumes was eagerly welcomed by the geological students of the Continent, as well as of our own country; and it was a constant source of congratulation to the unwearied author that he had lived to see the successful completion of his toils.

The pages of the Journal of the Royal Institution of Cornwall had been for many years open to his contributions; and in 1870 the members of that body showed their appreciation of his labours by electing him to the Presidential Chair. Although at this date he was much enfeebled in health, he discharged the duties of his office with untiring vigour, and presented to the Society several addresses, compiled with the greatest care and attention, on subjects which had occupied his thoughts for many years. The portion of his Address in 1871 which related to metalliferous deposits was reprinted by the Miners' Association of Cornwall and Devon, and afterwards translated into French and published in the 'Annales des Mines.' A paper which he contributed to the same Society in 1874, on the Detrital Tin-ore of Cornwall, was also published in France entire, in the 'Annales des Mines,' and extracts from it in a separate form by M. Zeiller.

Of the award of the Murchison Medal to Mr. Henwood, in recognition of his long-continued and valuable researches, I have already spoken. It only remains to add that he was never married, and that his death, which was sudden though not unexpected, took place at Penzance on the 5th of August last.

By the death of Mr. William Sanders, F.R.S., on the 12th of November, 1875, the Society has lost another of its early members, and one who for upwards of forty years of his life was intimately associated with the most distinguished men connected with geological science.

Mr. Sanders, who was born on the 12th of January, 1799, was a native of Bristol; and to the study of the geology of the neighbouring country he devoted his life.

At the commencement of his scientific career he was the friend and companion of Prof. Phillips in his Geological Survey of North Devon and Cornwall. But his principal work was the preparation and construction of an elaborate geological map of the district comprised within the Gloucestershire and Somersetshire Coal-field, on the scale of four inches to the mile. This work, which extended over fifteen years, was undertaken at the instigation of Sir Henry De la Beche and Prof. Phillips. Besides this map, he published some measured sections of the extensive cuttings on the Bristol and Exeter Railway, and on the line from Bristol to Bath. On these sections, which were drawn to scale, every detail, however minute, that was of any Physical or Palæontological importance, is

accurately delineated; and their value remains undiminished even after the lapse of 35 years.

Mr. Sanders, by his intimate knowledge of the Mendip area, was able to render valuable assistance to his native town in connexion with its water-supply, and also during the survey of the city with reference to its sanitary condition. He was also for upwards of thirty years the Honorary Secretary to the Museum of Natural History attached to the Philosophical Society and Institution of Bristol, and spared neither time, trouble, nor expense in carrying out its legitimate objects. Those who visited Bristol at the last meeting of the British Association will remember the just pride which he took in the geological collections in the Museum. Mr. Sanders was also an ardent student of Mineralogy and well versed in Crystallography.

He became a Fellow of this Society in 1839, and of the Royal Society in 1864. Several papers on geological subjects were read by him before the British Association between the years 1840 and 1849; but during his 36 years of Fellowship Mr. Sanders did not communicate any memoir to this Society.

The Venerable Archdeacon W. EDWARD HONY was elected into this Society no less than sixty-three years ago, in the year 1813, his certificate being signed by G. B. Greenough, H. D. Conybeare, H. Grey Bennett, and W. Rashleigh. He was at that time a Fellow of Exeter College, Oxford; and six years later we find his name among the proposers of Sir Charles Lyell, who, it will be remembered, was also of the same college. The only scientific paper of which he appears to have been the author, consisted of "Geological Remarks on the vicinity of Maestricht," communicated to this Society 1814, and published in our Transactions.

The Right Hon. Sir EDWARD RYAN, F.R.S., was a Fellow of this Society for a period of nearly thirty years, having been elected in 1846. Though he never communicated any paper to our publications, he took a warm interest in the progress of Geology, as well as in that of other sciences. He was born in the year 1793, and having taken his degree at Cambridge in 1814, was called to the bar in 1817. In 1826 he received the appointment of Puisne Judge of the Supreme Court of Calcutta, and in 1833 that of Chief Justice of the Presidency, which he held until 1843, when he returned to England. His resignation of the Chief

Justiceship did not, however, involve his retirement from public life; for, within a short time of his arrival in England, he became a member of the Judicial Committee of the Privy Council, a Railway Commissioner, and Assistant Comptroller of the Exchequer. In 1855 he became a member of the first Board of Civil-Service Commissioners, of which body he was for many years the guiding spirit. He was also one of the Vice-Chancellors of the University of London, in the welfare of which he took the warmest interest. His death took place on the 22nd of August last, after a short illness, and within six months of the time when he followed the remains of his old and attached friend Sir Charles Lyell to their resting-place in Westminster Abbey.

Colonel R. Baird Smith, C.B., of the Bengal Engineers, was elected a Fellow of this Society in 1845, having already, in 1842, communicated a memoir to our Proceedings, "On the Structure of the Delta of the Ganges, exhibited by the boring-operations in Fort William, 1836–40." His other geological papers, "On the crystalline structure of the Trap Dykes of the Sienite of Amboor, with an inquiry into the causes to which this peculiarity of certain igneous rocks is due," "Notes illustrative of the Geology of Southern India," "Economic Geology," and various memoirs on earthquakes, are published in the 'Madras Journal,' the 'Journal of the Asiatic Society of Bengal,' and the 'Calcutta Journal of Natural History.'

Mr. T. G. B. Lloyd, who was, until quite recently, a frequent attendant at our meetings, and who was well known to, and greatly esteemed by many of our Fellows, died on the 3rd of the present month. Mr. Lloyd was the eldest son of Dr. Lloyd, of Birmingham, a very old Fellow of this Society, and was born on the 15th August, 1829. He adopted the profession of a Civil Engineer, and was engaged in railway construction and surveying in Spain and in the United States and Canada. During the last three years of his life he was occupied in surveying lands in Newfoundland, in search of mineral property; and his companion in two of these expeditions, Mr. John Milne, F.G.S., communicated to this Society the geological results of their joint investigations. Mr. Lloyd's own contributions to our Proceedings consist of a valuable paper "On the superficial Deposits of portions of the Avon and Severn Valleys and adjoining districts," published in the Journal for 1870, and some "Geological notes from the State of New York," an abstract of which was passing through the press at the

time of his death. During his visits to Newfoundland Mr. Lloyd took great interest in the investigation of the scanty documents and traditions relating to the extinct Beothucs, or Red Indians of that island, and of the few material traces of their existence still extant; and upon these researches he founded three very interesting papers which were communicated to the Anthropological Institute and published in their Proceedings. He had also made considerable progress in an investigation of the characters of recent and fossil beavers; but his materials upon this subject are left in an incomplete state. He became a Fellow of this Society in 1864.

Mr. Nathaniel Plant, F.R.G.S., was born at Leicester in 1832. After leaving school, his taste for natural history led to his appointment as Curator to the Museum of the Philosophical and Literary Society in that town, which office he held about five years. Desiring, however, to learn something more of the great world of nature than could be found in a provincial museum, he went abroad in the year 1851, to Rio Grande do Sul, Brazil, and for fifteen years was actively engaged in collecting specimens of natural history, and in mining and geological explorations, during which time he discovered the existence of valuable minerals, ores, and coal in that part of Brazil. The President of Rio Grande do Sul procured for him an official appointment to draw up reports upon the mineral resources of the province, which were published in the Rio-Grande papers. They include descriptions of porphyries, copper, ironstone, petroleum-shale, and coal. He completed a report upon the great coal-deposit at Candiota. Prof. Agassiz, when at Rio de Janeiro, examined the fossils obtained from this coal-basin, and determined their similarity to those of Cape Breton; an account of these coals and fossils is published in our Journal. On his return to England in 1867, he was elected a Fellow of this Society, and became occupied with a futile endeavour to make the coal-deposits of service to the mercantile commerce of Brazil. He died in London in August 1875, at the age of 43.

Mr. Marcus Watson Tate Scott, who was elected a Fellow of this Society in 1852, was born at Belford in Northumberland, in September 1814. When eighteen years of age he removed to Newcastle-on-Tyne, where he entered the offices of Mr. Sopwith to learn the profession of a Mining Engineer and Surveyor, which he practised for several years in Newcastle. In 1845 he came to London, continuing in the active exercise of his profession until

his death, which took place when engaged in mining business in Shropshire, on April 6, 1875.

He was remarkable for extreme accuracy in whatever he undertook; his strict integrity gained him confidence and esteem; and his private character was of great amiability. He frequently attended the Meetings of the Society; and a communication made by him in May 1861, "On the 'Symon Fault' in the Coalbrook-Dale Coalfield" (see Quart. Journ. Geol. Soc. vol. xvii.), shows how carefully he applied his geological knowledge to practical mining.

Mr. James Middleton, formerly Principal of the College at Agra, who was elected a Fellow of this Society in 1844, died in the early part of last year. His activity lay principally in the direction of chemistry; and his chief researches related to the presence of fluoride of calcium in bones both recent and fossil. Upon this subject he communicated papers to the Chemical Society and to the 'Philosophical Magazine' in 1843 and 1844; and his sole contribution to the publications of this Society, which was read in the latter year, is "On Fluorine in Bones, its Source and its application to the determination of the Geological Age of Fossil Bones." In this paper he gave the analysis of certain fossil bones and of the bone of a Greek, the age of which could be fixed at about 2000 years, and showed that the amount of fluoride of calcium contained in them increased with their antiquity, being probably derived from water in contact with the bones. From this he inferred the age of the Sewalik fossils to be about 7700 years, and that of an *Anoplotherium* about 24,200 years. Mr. Middleton further communicated a paper "On the Specific Gravity of Seawater" to the Asiatic Society of Bengal, of which he was a member, and an analysis of a cobalt ore from Western India to the Chemical Society.

Mr. Frederick Erasmus Edwards must receive some mention at my hands in this catalogue of our losses during the past year, although he had retired from the Society for some time before his death, which occurred on the 15th October last. Mr. Edwards, who was born on the 1st October 1799, was one of the founders of the old "London-Clay Club," from which the "Palæontographical Society" originated; and to the memoirs published by the latter he contributed those valuable monographs on the Eocene Mollusca which are so well known to geologists. The publication of these com-

menced in 1848, and terminated in 1858. Mr. Edwards also communicated papers on fossil Eocene Mollusca to the 'London Geological Magazine,' the 'Geologist,' and the 'Geological Magazine,' but never contributed any thing to our 'Proceedings.' He became a Fellow of this Society in 1836, and resigned his Fellowship in 1841; joined us again in 1861, and finally withdrew in 1871. His large collections have been secured for the British Museum.

There is yet another geological veteran of whose loss some notice ought to be taken, though at the time of his decease he had retired from this Society. I mean Mr. NATHANIEL THOMAS WETHERELL, who was well known to many of our body, and who, on more than one occasion, contributed papers to our Proceedings. He was a Member of the old "London-Clay Club," and devoted particular attention to the fossils of the London Clay, especially to those found to the north of London, and also to the fossils of the Glacial Drift deposits of Finchley and Muswell Hill. The collection he formed from the latter localities is now in the Jermyn-Street Museum, while the greater part of his extensive collection from the London Clay has been secured for the British Museum.

The papers he communicated to this Society were:—"Observations on the London Clay of Highgate Archway," 1832; "On an *Ophiura* found at Child's Hill, to the north-west of Hampstead," 1833; "Observations on a Well dug on the south side of Hampstead Heath," 1834; "A notice of some undescribed Organic Remains which have been recently discovered in the London Clay Formation," 1839; "On a species of the genus *Bulimus* occurring in the London Clay of Primrose Hill," 1846; and "On the occurence of *Graphularia Wetherellii* in nodules from the London Clay and the Crag," 1858.

In addition to these, Mr. Wetherell furnished numerous papers to the 'Magazine of Natural History,' the 'Philosophical Magazine,' the 'Geologist,' the 'Geological Magazine,' and to the Proceedings of the Geologists' Association. They relate for the most part to discoveries in his favourite formation, the London Clay, though occasionally his investigations went as low as the Chalk and the Greensand.

He died at his residence at Highgate, where he was in the practice of the medical profession, on December 22, 1875.

At our last Anniversary, when I had the honour of addressing you from this Chair, we had but recently been installed in these apartments, and I took occasion to institute some comparisons between the condition of our Society at that time and its position some forty-six years previously, when first it removed to Somerset House. My retrospect must to-day be confined to the past year, during which, as you have already heard, we have lost so many and such valued Members; and I shall, in the few remarks which it is my duty to address to you, direct your attention to the present and the future rather than to the past: and first I would speak as to the main objects which it appears to me that this Society ought to have in view, and the means at its command for attaining them.

We constitute, in the words of our Charter, "a Society for investigating the Mineral Structure of the Earth;" and certainly, if we compare the present state of Geological science with that of the year 1825, when our Charter was granted, and consider the proportion of the advance in knowledge which during the last fifty years has been due to the labours of Fellows of this Society, it must be conceded that its existence has not been in vain. This very advance in knowledge, however, in some measure appears to have a deterrent effect upon those who are entering upon Geological pursuits. The field is so great, and has been so laboriously worked, that it seems impossible for any one mind to grasp its whole extent; and it is not every one who can content himself with limiting his researches to some single special department. There appear, moreover, to be no longer any of those grand discoveries to be made which gladdened the heart of many of our earlier Geologists, and returned them so ample a reward for their labours. And, further, the recognition by the State of the importance of Geology in connexion with the material prosperity of the country has called into existence a body of trained Geologists, who are devoting their entire life and energies to the subject, and with whose knowledge and experience it appears to the amateur, when first beginning his studies, that it will ever be utterly impossible to compete. In all this there is, no doubt, a certain amount of truth; but practically these objections will be found but of little weight; for experience has shown, and I trust will long continue to show, that the difficulties which so forcibly present themselves to the mind of the beginner are for the most part apparent rather than real, and, where real, are by no means insurmountable. We find as a fact that great discoveries in Geology are still being made, and that there is ample room in the field for those to whom the

science is of the nature of a relaxation for leisure hours, as well as for those whose lot it is to regard it more as a professional pursuit. There is, moreover, no real competition between the two classes of investigators; and while those who have had the greater scientific and professional training are in all cases ready to lend a helping hand to the amateur Geologist, there are none more willing to acknowledge the value and importance of the corps of volunteers from whose local or general knowledge they have so often derived benefit, and whose labours have so frequently and materially lessened their own. It is not, of course, to those among us who have for years been Fellows of our Society, and to whom so much honour is due for their researches, that any such remarks as these are directed. It is rather to those who in such large numbers have lately joined our body, and to those who are still doubtful as to the advantage of doing so, that I address myself; and I do so in the hope of encouraging them to persevere in their study of our subject, and of inducing some of them, at all events, to join in the number of those who contribute memoirs to our Proceedings.

The Society is much to be congratulated on the large accession to its numbers which has of late years taken place; for one of our main objects ought ever to be to foster and perpetuate the taste for Geology throughout the country, and to facilitate the entry of all deserving students into our body. Young Geologists must not, however, expect that it lies within the province of the Society directly to impart elementary knowledge. Our meetings are held for the purpose of having new facts, or new views of the bearing of admitted facts, or corrections of important errors, brought before us for discussion, and not for mere elementary instruction, though many of us could probably testify as to how much may be incidentally learned through a constant attendance at our meetings. Another great advantage our Society affords is that of enabling those who are interested in the same pursuit to meet together at stated intervals, and to form acquaintance with each other. There is, I believe, no better means of advancing the interests of a science than that friendly intercourse between its followers which, I venture to say, is one of the most excellent characteristics of this Society. Another of the means at our command is the possession of a well-stored Library, the contents of which are accessible to all our Members, and the due sustentation of which appears to me by no means the least important of our functions. We possess at the present time about 10,000 volumes, and are yearly adding about 300 to the number, either as presents, or by

means of exchange of our own publications, or by purchase. It is here that the beginner will find accumulated stores of knowledge for his assistance; while, should those of more experience point out deficiencies on our shelves, there can be but few ways in which the funds of the Society could be better employed than in making them good.

Our Museum * is intended rather for a repository of specimens illustrative of papers read before the Society and of foreign Geology than for the general purposes of a Geological Museum. The propinquity of the British collections in the Jermyn-Street Museum, and the collections so readily accessible in the British Museum, render the existence of a third Museum of the same character unnecessary. There are, however, in our cabinets many specimens of interest in connexion with the history of Geology; as, for instance, the fossils figured and described in 'Siluria,' and in some of Sir Roderick Murchison's other writings, and specimens illustrative of classical memoirs by Webster, Fitton, Buckland, Mantell, and others. We have also more than one collection of typical specimens. The way in which our Museum may be made of most service in advancing our science will be by our Fellows enriching it with foreign specimens, and thus rendering it more complete in a department which is beside the purpose of one of our national museums, and which, so far as geographical arrangement is concerned, is of secondary importance in the other.

For the advancement of knowledge, however, our publications must take the first place; and the reputation which our Quarterly Journal bears abroad affords good evidence of its value. To perpetuate its high character, not only must a judicious selection be made from the papers submitted to the Society, but illustrations must be provided with no niggardly hand. With regard to the former point, it will be well for Fellows to bear in mind that many communications of great local value may not be of sufficient general interest to render their publication *in extenso* desirable, and that purely speculative questions, however well adapted to raise an interesting discussion, are, like those relating to merely elementary matters, hardly within the scope of the Society's Journal. For questions of temporary interest the pages of the Geological Magazine and other scientific serials are always open; while the excellent publications of the Palæontographical Society relieve us of a duty which we might find some difficulty in fulfilling.

* For an interesting notice of the Museum of the Society, see 'Nature,' Jan. 20, 1876.

As stimulants and rewards to those who are engaged in investigating the mineral structure of the earth, the medals and funds which our former chiefs, Wollaston, Murchison, and Lyell, have left at our disposal, and that which our venerable member Dr. Bigsby has just founded, are of the highest value. While the medals are justly regarded as among the greatest honours to which a geologist can aspire, the Donation Funds may, and, I hope, generally do combine a complimentary appreciation of a fellow-worker's labours, with a not unwelcome aid in prosecuting them.

We have, then, at our command much that will forward the interests of our science; but what are its present prospects? what is being done in its behalf? in what manner are recent discoveries in other branches of knowledge likely to affect it? and in what direction are future discoveries likely to be made, and the conclusions modified which are at the present time generally accepted?

He would be a bold man indeed who would venture to answer these latter questions with any degree of assurance; and yet, in considering what is going on around us, we cannot altogether refrain from some prognostications of the future. Looking, for instance, at those researches into Solar Physics which the marvellous powers of spectrum analysis and solar photography have rendered possible, and with which the names of Mr. Norman Lockyer and Mr. Warren De la Rue are so intimately associated, who cannot but feel that they have a close and intimate bearing upon the early history of the earth? Prof. Prestwich, in his Inaugural Lecture at Oxford on the Past and Future of Geology, has, with the aid of Mr. Lockyer, discussed this subject at some length. I need, therefore, but briefly state one or two of the principal points referred to, and call attention to the inferences which they suggest. In the first place, the resemblance in the constitution of the sun and of the earth is such as to support the view that, in the main, they consist of the same elements. Out of the sixty-four terrestrial elements, twenty have been traced in those parts of the solar atmosphere which are known as the 'chromosphere' and the reversing layer. Nor, with possibly two exceptions, does the spectroscope afford any indication of unknown elements.

But, in the next place, throughout the solar atmosphere, which is of enormous height, and consists of hydrogen and of metallic elements in a gaseous state, there is a tendency for the various vapours to arrange themselves in layers in an order which Mr. Lockyer finds to correspond with the old atomic or combining weights, and not with

the new. He has found reason to suppose that the metalloids or non-metallic elements are, as a group, placed outside the metallic atmosphere of the sun; while the metals of the tungsten, antimony, silver, and gold classes, have not as yet been traced. His suggestion with regard to the probable structure of the crust and nucleus of the earth is, that owing to the localization of the elements as before mentioned, they would, on cooling, occupy to a great extent the same relative positions as they did when in the gaseous state. We should thus find oxygen, silicon, and other metalloids, which once formed an outer atmosphere, combined mainly with the higher metals to form a crust; lower down would be iron and its associated group of metals; and, finally, an inner nucleus, containing the other and denser metals, not attacked by any metalloid, because the higher metals had already had the opportunity of combining with them. Prof. Prestwich considers that the relations of the two great classes of fundamental igneous rocks which underlie the sedimentary strata, and which formed originally the outer layers of the crust, afford evidence of the correctness of this hypothesis. If, as appears to be the case, there exists an upper layer of granite and other Plutonic rocks rich in silica, with a moderate proportion of alumina, and poor in iron and magnesia, and a second layer consisting of a lower mass of basaltic and volcanic rocks of greater specific gravity, with silica in smaller proportions, alumina in equal, and iron, lime, and magnesia in much larger proportions—if in the lower layer these elements are mixed with a greater variety of others, as occasional constituents, than are found in the upper layer—and if the denser metals are found in much larger proportion in the more central portion of the nucleus than in the outer crust, all these phenomena would be in accordance with such an origin. If such a view of the origin of the crust of the earth were accepted, it would materially strengthen the assumption that the fluidity of those volcanic rocks, which still from time to time are ejected upon the surface of the earth, is not due to any recent chemical or mechanical cause, but may rather be traced to their original heat at the time of their passing from the gaseous state. If, however, matter still retaining its original fluidity exists in the manner here supposed, it is impossible to reconcile such a state of things with the generally accepted conclusions of our most distinguished mathematicians and astronomers as to the present constitution of the earth and the thickness of its crust. It is true that such a question relates rather to cosmogony than geology; but the nature of the inner constitution

of the globe has so intimate a connexion with what, during geological time, has taken place upon its surface, that it must be regarded as within our province; and indeed the subject is one which of necessity is constantly forcing itself upon our attention. The enormous elevations and depressions of the surface which have taken place all over the globe, the vast faults and dislocations of the strata, the contortion and occasional inversion of beds of great thickness, and many other geological phenomena seem more in accordance with a comparatively thin and yielding crust, than with one of such great rigidity and thickness as to satisfy the requirements of Mr. Hopkins or Sir William Thomson.

Nor is it only with regard to theoretical considerations as to the pristine condition of our globe, that spectrum-analysis, to which our knowledge of Solar Physics is mainly due, seems likely to prove of service to those engaged in geological and mineralogical investigations. To the metallurgist especially it is by no means improbable that it may eventually become almost indispensable, as it may, in the examination of metals, make known the presence of foreign elements when in such minute quantities as to be almost beyond the reach of the ordinary methods of analysis. At present, however, the researches in this direction have been, in the main, confined to the examination of gold; but the success of one of our Fellows, Mr. W. Chandler Roberts, F.R.S., in the quantitative analysis of gold-copper alloys, justifies a hope that the process may ere long be found of useful application in other cases, as, for instance, in that of argentiferous lead.

The researches of modern chemistry and the greater facilities now afforded for exact microscopic observations bear so evident a relationship to the advancement of mineralogical knowledge that I need hardly mention them among the sources to which we may look for accessions to the domain of our science. Some of the discoveries, however, which have been made during the cruise of the 'Challenger' suggest the imperative necessity of laying the utmost importance on chemical considerations, even when seeking to account for the existence of what appear to be ordinary sedimentary deposits. The remarkable correspondence between the white, or *Globigerina*-ooze, of the Atlantic and Southern Seas and our Upper Chalk is of course well known, though recent discoveries have thrown much additional light upon the subject. So long ago as 1849, Sir Charles Lyell, in his 'Second visit to the United States,' related an anecdote in connexion with this subject which it may be worth while to

repeat. He mentions how, in the Museum at Washington, he saw a number of objects collected by the Exploring Expedition conducted by Capt. Wilkes,—among them masses of limestone found in recent coral reefs, one of which, as white and soft as chalk, had been brought from the Sandwich Islands, and might have been mistaken for a piece of Shakespeare's cliff near Dover. It reminded Sir Charles that an English friend, a Professor of political economy, met him about fifteen years previously on the beach at Dover after he had just read the 'Principles of Geology.' "Show me," he exclaimed, "masses of pure white rock like the substance of these cliffs, in the act of growing in the ocean over areas as large as France or England, and I will believe all your theory of modern causes." "Since that time," wrote Sir Charles, "we have obtained data for inferring that the growth of corals and the deposition of chalk-like calcareous mud is actually going on over much wider areas than the whole of Europe; so that I am now entitled to claim my incredulous friend as a proselyte."

But we now find modern causes at work reproducing other old rocks than those of the nature of the Upper Chalk. Prof. Wyville Thomson tells us that during the southern cruise of the 'Challenger' the sounding-lead brought up no less than five absolutely distinct kinds of sea-bottom, without taking into account the stony deposits in the neighbourhood of land. Of these, one consists of volcanic mud and sand; while the other four appear to be of purely organic origin, and, though for the most part differing greatly in character, seem all to be in process of deposition at the present time. These organic deposits are as follows:—

A. A greenish sand which, on examination with the microscope, is found to consist almost exclusively of the casts of Foraminifera in one of the complex silicates of alumina, iron, and potash—probably some form of glauconite. *Globigerina*, *Orbulina*,' and *Pulvinulina* are represented in the casts, but not in such abundance as *Miliola*, *Biloculina*, and several other forms. This kind of bottom was found but rarely, the two soundings mentioned being 98 and 150 fathoms; and the deposit is regarded as evidently exceptional, and depending on some peculiar local conditions.

B. "*Globigerina*-ooze," consisting of little else than the shells of *Globigerina*, whole, or more or less broken up, with a small proportion of the shells of *Pulvinulina* and of *Orbulina*, and the spines and tests of Radiolarians, and fragments of the spicules of sponges. At times there is an admixture of fine granular matter,

filling the shells and the interstices between them, which, under a high power of the microscope, is found to consist of "coccoliths" and "rhabdoliths," the remains of those singular organisms "coccospheres" and "rhabdospheres," which are found to live abundantly at the surface, especially in the warmer seas. The *Globigerina*-ooze is rarely met with at a depth exceeding 2250 fathoms.

At a greater depth it was found, almost universally, that the calcareous formation gradually passes into and is finally replaced by D, an extremely fine pure red clay, which occupies over large areas all depths below 2500 fathoms, and consists almost entirely of alumina and a silicate of the red oxide of iron. The transition from the *Globigerina*-ooze into this class of deposits is very slow, and extends over several hundred fathoms of increasing depth, the shells gradually losing their sharpness, and assuming a kind of rotten look and a brownish colour. They also become more and more mixed with a fine amorphous red-brown powder, which increases steadily in proportion until the lime has almost entirely disappeared.

Intermediate between the white *Globigerina*-ooze (B) and the red clay (D) is a grey ooze (C), which appears to be rather a transition-bed than one with such well-defined characteristics as B and D. It occurs at an average depth of 2400 fathoms, but is also occasionally found at considerably greater depths.

One of the remarkable features in the case is the large area over which the red clay (D) is being deposited. Between the island of Teneriffe and Sombrero, a distance of about 2700 miles, there was a tract of 1900 miles over which the soundings gave red clay, and of only about 720 miles where they brought up *Globigerina*-ooze.

As to the origin of the red clay, Prof. Wyville Thomson considers that there is little reason to doubt that it consists of the insoluble portion of the same calcareous organisms the remains of which constitute the *Globigerina*-ooze—the soluble portion, consisting of about 98 per cent. of carbonate of lime, having in some manner been removed. Though the exact mode in which this has been effected seems at present involved in mystery, he suggests that a great part of the bottom water in the deep troughs in which the red clay occurs was last at the surface in the form of circumpolar freshwater ice, which though fully charged with carbonic acid, may possibly have been comparatively free from carbonate of lime, so that its solvent power was thus greater.

Dr. Carpenter, while agreeing with Prof. Wyville Thomson that

solution by carbonic acid is the most probable reason for the disappearance of calcareous shells in a deep-sea deposit, where no mechanical action can be invoked, cites the observations of our Fellow, Mr. H. C. Sorby, F.R.S., on the increase of solvent power for carbonate of lime possessed by water under greatly augmented pressure; and certainly under a pressure of from 500 to 600 atmospheres, or upwards of three tons to the inch, this cause might well apply. Dr. Carpenter doubts, however, whether the relation between the red clay and the depth of the sea is so constant or universal as is thought by Prof. Wyville Thomson—and, what is more important, is inclined to disbelieve in the existence of insoluble argillaceous matter, even to the extent of two per cent., in the calcareous shells from which the *Globigerina*-ooze is derived. He considers that its presence is more probably due to a *post-mortem* deposit in the chambers of Foraminifera than to the appropriation of the material for the clay by the living animals for the formation of their shells. In support of this view, he calls attention to the replacement of the sarcodic bodies of the animals—such as is now going on in the districts where the deposit A is being formed, and which Prof. Ehrenberg pointed out, in 1853, had also taken place at the period of the Greensand formation,—and shows that not improbably the glauconite may have been converted into clay in the presence of an excess of carbonic acid. It is possible that both causes may have been at work, and that, while the calcareous shells have not been composed of carbonate of lime absolutely free from any iron, silica, or alumina, yet that during the decomposition of the sarcode after death a certain amount of glauconite was deposited, itself to be eventually metamorphosed into clay.

However this may have been, we have this important discovery laid before us, that there are existing causes at work which at a great distance from land, and without the aid of any transported sediment arising from denudation, suffice for the tranquil deposition of beds of red clay on the bottom of a deep sea, these beds shading off under different circumstances into deposits closely resembling our Lower and Upper Chalk and Upper Greensand formations. It will be for the geologist to determine whether not only these rocks, but others of older date, and especially Clays and Slates, may not have their origin assigned to the action of similar causes.

It has long been known that many, if not most, of the beds of red clay overlying the Chalk and various Limestone formations were the insoluble residue of the rock left near the surface, after the cal-

careous portions had been dissolved away by the action of water charged with carbonic acid. It has also been noted that these argillaceous beds were more fully developed in some places than in others. Confining ourselves for the moment to the Cretaceous rocks, we may now, I think, connect, within certain limits, the amount of clay present in any case with the depth and the circumstances under which the rocks were deposited, and find reason for supposing, until some good evidence is produced to the contrary, that the grey chalk which, from the amount of alumina present, produces, when burnt, an hydraulic lime, owes its properties to the depth of the ocean under which it was formed, and to the amount of Polar water with which it was brought into contact, and that the white Chalk over which the red clay, or "chemical drift," as it has been termed, appears in greatest force, may have been deposited in deeper water than that over which it is found but in minute quantity. We may further connect the Greensand, the careful microscopic examination of which will no doubt be renewed, with those shallower and peculiar oceanic conditions under which the deposit of glauconite during the decomposition of organic matter seems most readily to take place. Which of the older red clays are of chemical and not of sedimentary origin, and whether there are any blue or green clays which owe their existence to similar causes, but in which the oxidation of the contained iron has, for some reason, not proceeded so far, are questions for the chemical geologist. Professor Huxley has already called attention to the importance of this discovery with regard to the earliest known formations, and pointed out how unsafe is the inference from the absence of recognizable organisms in the rocks, that no life existed at the time of their deposit. The whole tendency of the evidence of Palæontology goes, he says, to prove that the earliest forms of life must have been vastly simpler than the fossils as yet met with in the earliest sedimentary strata. Much as metamorphic action has done to destroy all traces of organisms in the older rocks, it still seems possible that patience and a microscope may, in this as in other instances, reveal to us as yet hidden treasures.

The abundance of manganese over the red-clay area of the Atlantic is another subject for investigation; nor will the greater prevalence of siliceous Diatoms near the Poles and in some other parts of the Atlantic be without its significance to those who are interested in the history of flint in Chalk and in other calcareous rocks.

Since our last anniversary another British expedition has set

forth upon its travels, to the return of which geologists will look forward with as much interest as they do to that of the 'Challenger:' I mean the Arctic Expedition, under the command of Capt. Nares. Unfortunately no professional Geologist is attached to his staff of officers; but there are among its members some to whom geological observation is by no means unfamiliar, and who will, at all events, lose no opportunities of collecting any fossil remains which can be brought home for determination. Apart from any stratigraphical or palæontological details, it may be hoped that some of the physical observations with regard to the behaviour of large masses of ice, as distinct from that of ordinary glaciers, may be instructive. This is, indeed, a subject on which Prof. Nordenskiöld has already made some remarks. The question of the present climate and proportions of land and water, and of the past geological conditions of the circumpolar area, is also one the solution of which involves geological considerations of great moment. Through the researches of Prof. Oswald Heer we have been made comparatively well acquainted with the fossil flora of the arctic regions, and especially with that which is referred to Miocene times, and which comprises upwards of 350 species.

Of these Arctic species nearly 100 occur in the Miocene flora of Europe, the percentage being least in Spitzbergen and greatest in Greenland, while of the European fossil floras that of the Baltic near Dantzic (55° N.) comes nearest to the Arctic, having about 54 per cent. of the same plants.

Prof. Nordenskiöld states that among these Miocene plants at Cape Lyell, in Spitzbergen, are found the *Taxodium distichum* (the swamp-cypress of Texas), Sequoias of enormous size allied to the *Wellingtonia* of California, large-leaved birches, limes, oaks, beeches, planes, and even magnolias.

And Prof. Heer, arguing from the fossil flora of Atanekerdluk, on the Waigat, opposite Disco, in lat. 70°, infers that, beyond a doubt, North Greenland in the Miocene age had a climate warmer by at least 30° Fahr. than at present. He shows that of many of the trees the nearest living representatives are not to be found nearer than 10° or even 20° further south, and that one of the most important, a *Daphnogene*, with large thick leathery leaves, was probably an evergreen. Of other plants, there are some of which it is quite certain that they could not have withstood a low temperature. Even assuming that such forms were living close to the highest northern limit compatible with their existence, the occurrence of

the trees just cited in Spitzbergen, about 8° further north, shows that the beeches and planes could flourish in that high latitude; and inasmuch as at the present day firs and poplars reach to a latitude of 15° above the artificial limit of the plane, and 10° above that of the birch, it is inferred that there was nothing in the Miocene climate to prevent firs and poplars from growing as far northward as land existed. The *Sequoia Langsdorffii* has its nearest living representative in the *S. sempervirens* (Red-wood), a tree the present northern limit of which is about lat. 53°, and which requires for its existence a summer temperature of 60° F., and for its fruit to ripen one of 65°. The winter temperature for this tree must not fall below 31°; and that of the whole year must be at least 50°; but the present annual temperature at Atanekerdluk is about 20° F., and this is somewhat above the normal temperature for the latitude. Further to the east, at Attenfiord, a temperature of 33° Fahr. is met with; but even this abnormal temperature for so high a latitude is 17° lower than what we are obliged to assume as having prevailed in Spitzbergen during the Miocene period. Professor Heer regards such facts (which are, moreover, mere links in the grand chain of evidence obtained from the examination of the Miocene flora of the whole of Europe) as convincing. They prove to us, he says, that we could not by any rearrangement of the relative position of land and water produce for the northern hemisphere a climate which would explain the phenomena in a satisfactory manner. We must only admit, he adds, that we are face to face with a problem whose solution in all probabity must be attempted, and, we doubt not, completed by the astronomer.

It might, indeed, be urged that we are hardly justified in restricting the growth of particular varieties of such trees as the fir, the poplar, and the birch to the conditions under which they are at the present time able to exist. In addition, however, to the extreme cold, we must remember that it is also with a prolonged absence of sunlight that the plants which formerly flourished in such latitudes as 80° would have to contend; and even if there were a sufficiency of light, it is hard to conceive in what manner thick-leaved evergreens, such as the *Daphnogene*, could endure the climate, or Sequoias ripen their seed.

This warmth of climate, moreover, within the arctic circle, which it appears necessary to admit as having prevailed there in Miocene times, seems also to have been enjoyed by those regions even in a higher degree during earlier periods of the earth's history.

During the Upper Cretaceous period deciduous trees and dicotyledons flourished at Atanekerdluk, as well as two species of *Magnolia* and a fig, of which not only the leaves but the fruit are preserved in the fossil state. I have myself seen the specimens, which were obligingly shown to me at Stockholm by Prof. Nordenskiöld, from whose paper * on the former climate of the polar regions I am largely quoting; and no product of the far north ever appeared to me more striking.

In the Lower Cretaceous period, however, the climate seems to have been still warmer. Among 75 species distinguished by Prof. O. Heer, there are 30 ferns, 9 Cycadeæ, and 17 Coniferæ; and of the ferns one third part belong to the genus *Gleichenia*, which still flourishes in the neighbourhood of the tropics and the warmer parts of the temperate zone. The same holds good of the Cycadeæ, most of which are referable to the genus *Zamia*, while some of the Coniferæ are nearly related to forms still existing in Florida, Japan, and California. Judging from the vegetable remains, Prof. Heer infers that the Arctic climate of the early part of the Cretaceous period was much like that which now prevails in Egypt and the Canary Isles.

In Jurassic times the temperature seems to have been much the same, while the marine remains of the Triassic period, consisting of Ammonites, Nautili, and the great Saurian described by Mr. Hulke (the *Ichthyosaurus polaris*), all point to climatal conditions much warmer than those of the present day, and to a comparatively warm ocean.

The Coal-measures, with their profuse vegetation agreeing in character with that found in lower latitudes, the Mountain Limestone, built up with fragments of Corals and Bryozoa and shells of marine mollusca, all tell the same story.

To quote the words of Prof. Haughton, the arguments from the occurrence of coal-plants and Ammonites strengthen each other, the coal-plants rendering the question of *light*, and the Ammonites that of *heat*, insuperable objections to the admission of any received geological hypothesis to account for the finding of such remains *in situ* in latitudes so high as those of Melville Island, Prince Patrick's Island, and Exmouth Island.

The Spitzbergen fossils carry us, however, up to still higher latitudes, where we have a Lime, an Arbor-vitæ, and a Juniper nearly on the 79th parallel of latitude, or within less than 800 miles of the

* Geol. Mag. dec. ii. vol. ii. p. 525.

actual Pole. We can hardly believe that such trees could grow on the confines of all arboreal life; and when we consider that the difference in latitude between Spitzbergen and the Pole is just the same as between Cornwall and the Shetland Isles, it is hard to believe that at the time when such comparatively southern forms were flourishing in Spitzbergen, the Pole itself could have been so ill-adapted for all vegetable life of the kind as it must be at the present day. Among other suggestions which have been offered as a means of accounting for the phenomena, that of a change in the obliquity of the axis of rotation of the earth with regard to the ecliptic has been revived, and advocated with much ingenuity by Mr. Belt. As his views have been ably expounded by Mr. Henry Woodward in his Presidential Address to the Geologists' Association, I need not enter further into them. They have already been controverted by Mr. Croll, in his work on Climate and Time; and certainly, though with the axis of the earth vertical to the ecliptic there might be perpetual light at the Pole, it is hard to see how, with the sun always on the horizon, the inhabitants of that part of the world would be blessed with perpetual spring. Indeed, according to Mr. Croll, the aggregate quantity of heat received by the polar regions of a globe with its axis vertical to the ecliptic, would be far less than at present.

The three points which it appears to me are most important to bear in mind with regard to the Arctic flora are:—1, that for vegetation such as has been described there must, according to all analogy, have been a greater aggregate amount of summer heat supplied than is now due to such high latitudes; 2, that there must have been a far less degree of winter cold than is in any way compatible with the position on the globe; and, 3, that in all probability the amount and distribution of light which at present prevail within the Arctic circle are not such as would suffice for the life of the trees.

Should the present Arctic expedition succeed in finding traces of what must be regarded as a temperate, if not indeed a subtropical fossil flora, like that of Greenland and Spitzbergen, extending to latitudes still nearer the Pole, it does appear to me that geologists will be compelled to accept as a fact that the position of the axis of rotation of our planet has not been permanent; and they will have to call upon astronomers to find some means of admitting what they now regard as impossible.

An astronomer and mathematician of no mean ability, the late

Sir John W. Lubbock, in a paper communicated to this Society in 1848, has speculated upon this subject, which was in consequence discussed by the late Sir Henry Delabeche in his Presidential Address in 1849.

Sir J. W. Lubbock remarked that the dictum of Laplace as to the impossibility of accounting for the changes which have taken place on the surface of the earth, and in the relative positions of land and water, by a change in the position of the axis of rotation, was founded upon the absence of two considerations, both of which appeared to him essential. These were—

1. The dislocation of strata by cooling,
2. The friction of the surface.

The latter consideration is apparently of but little importance; but with regard to the former, he pointed out how, if from any cause the axis of rotation did not coincide with the axis of figure*, the pole of the axis of rotation would describe a spiral round the pole of the axis of figure until it finally became, as it is at present, identical with it. He considered it unlikely that originally the axis of rotation should have coincided exactly with the axis of figure, unless the whole globe were perfectly fluid—but added that we might go back to a time less remote, when the earth was in a semifluid state, and in consequence of the different degrees of fusibility of different substances, was partly solid, in irregular masses; and, in consequence, the two axes did not coincide. We might, he added, assume the original state of want of uniformity between them to have been at a period even more recent, when the earth consisted of land and water, and was suited for the support of animal life. He then proceeded to show how, if, after any length of time the solid spheroidal part of the earth moved about any new axis of rotation, the water would occupy a new position about a new equator, land would become sea, and sea land, &c.

He added that, if the axis of the earth would suffer a displacement by reason of the causes which produce the precession of the equi-

* An axis of figure may, I presume, be defined as a principal axis of a body around which it would revolve *in æquilibrio*, and would therefore, when once the motion had been imparted, continue to do so for ever, if no extraneous forces acted upon it. The axis of rotation need not, at the time of the motion being imparted, correspond with the axis of figure, though the two would ultimately coincide. On the same principle a change in the form of a revolving body might displace its axis of figure, and for a time prevent the axis of revolution from coinciding with it.

noxes, we should have another and more natural way of accounting for the existing phenomena; but this has been held to be impossible.

I am not at present going to question whether this holding is correct; but with regard to Sir J. W. Lubbock's reasoning as to the necessity of the axis of figure coinciding with that of rotation, it appears to me of the greatest importance; for if it hold good, any alteration in figure cannot but have some effect on the position of the axis of rotation. No doubt, if the whole globe, or even the solid portion of it, were a regular spheroid, with a large equatorial protuberance, any modification on its surface would have to be on an enormous scale to produce any sensible effect upon its axis of revolution. But, after all, is the earth, strictly speaking, a spheroid? and are not some of the arguments and dicta based upon its spheroidal character founded on a fallacy? For it does appear to me a fallacy to treat as one homogeneous spheroid a body partly consisting of a mass of solid or quasi-solid matter of irregular form, and partly of a liquid mass in constant motion, irregularly distributed over a portion of its surface. No doubt the contour of the liquid portion is, according to established geometrical laws, almost that of a regular spheroid; but its distribution, except in the case of inland seas, can have but little to do with the regulation of the movement of the solid body on which it rests. It is true that Laplace has maintained that "whatever may be the law of the depth of the ocean, and whatever the figure of the spheroid which it covers, the phenomena of precession and nutation will be the same as if the ocean formed a solid mass with this spheroid;" but do the position of the axis of revolution, and its permanence in one spot, come under the same category as precession and nutation? It certainly appears to me that the position of the axis of revolution must mainly depend upon the form of the mineral portion of the globe, and be but in the slightest degree affected by the distribution of the ocean, the specific gravity of which is moreover only about one fifth of that of the more solid portion.

With regard to the permanence of the axis of rotation, if it must of necessity coincide with the axis of figure, and if the figure of the mineral portion of the earth, in consequence of upheavals and depressions, of the wearing away of continents and the transportation of their constituents by mechanical or even chemical means, is being constantly changed, so as to acquire a new axis, then the axis of rotation must also as constantly be undergoing a change of position.

Let us now glance at some of the irregularities of form of the

more solid part of the globe as at present existing. The difference between the polar and equatorial diameters of our globe has been calculated at about 26 miles, or about 13 miles in the radius; but at the equator itself, little more than one-fifth of the circumference of the globe is dry land, and nearly four fifths are sea; and this sea is by no means shallow, as the soundings taken by the 'Tuscarora,' the 'Challenger,' and other exploring vessels will prove. Leaving those taken near land out of the calculation, I find that 48 soundings in the Pacific, between 15° and 30° north latitude, give an average depth of 2634 fathoms, or 5268 yards—that is to say, within a few yards of three miles. The South Pacific does not appear to have been so well explored; but across the Atlantic, in the equatorial regions between 10° N. and 10° S., I find that an average of 32 soundings gives a mean depth of 2309 fathoms, or 4618 yards, while, in one spot in lat. 15° degrees S., Sir James Ross did not find the bottom with a line of 4600 fathoms, or nearly 5¼ miles. In the Indian Ocean, within the same limits, 20 soundings give an average of 2468 fathoms, or 4936 yards, or more than 2¾ miles. Taking these soundings as fair representations of the depth of the sea in the neighbourhood of the equator, it appears that we may at once reduce the equatorial diameter of the more solid part of the globe by from 5½ to 6 miles over nearly four fifths of its circumference; that is to say, we may reduce the usually accepted equatorial protuberance from about 13 miles to a little over 10. It is not within my province to inquire whether the fact of so large a portion of the equatorial protuberance being of so much less specific gravity than if it were composed of mineral matter will in any way affect the established calculations with regard to the precession of the equinoxes and the nutation of the poles, or, what is of more importance to us, the inferences with regard to the crust of the earth which have been thence deduced.

But while so large a portion of the surface of the land is, in the equatorial regions, so much below the normal level, there are, especially in the northern hemisphere, large tracts of land which, like the great plateau of Tibet, are some thousands of feet above it. The average elevation of the whole of Asia has, indeed, been estimated at 377 yards, or nearly a quarter of a mile above the sea-level. The depth of the ocean in non-equatorial regions must no doubt be taken into account; but practically, the spheroidicity of the globe, on which the stability of the pole has been held to depend, may be regarded as, even at the present time, considerably less than is usually supposed,

When, however, we come to think of the enormous elevations and depressions which some parts of the globe have undergone during geological time, it is by no means difficult to imagine conditions under which the general average, so to speak, of the surface, would approach much more nearly to the form of a sphere, and the globe would become much more sensitive of any disturbances of its equilibrium; but, whether the globe is a sphere or a spheroid, it is hard to see why disturbances of its equilibrium should not affect the position of its axis of rotation.

Taking our globe with the distribution of land and water as at present existing, I should like to inquire of mathematicians what would be the theoretical result of such a slight modification, geologically speaking, as the following:—Assume an elevation to the extent, on an average, of 4000 feet over the northern part of Africa, the centre of the elevation being, say, in 20° north latitude. Assume that this elevation forms only a portion of a belt around the whole globe, inclined to the equator at an angle of 20°, and having its most northerly point in the longitude of Greenwich, and cutting the equator at 90° degrees of east and west longitude. Assume that along this belt the sea-bottom and what little land besides Africa it would traverse were raised 4000 feet above its present level over a tract 20° in width. Assume further that the elevation of this belt was accompanied by corresponding depressions on either side of it, so as to leave the total volume of the mineral portion of the earth unaffected. Would not such a modification of form bring the axis of figure about 15° or 20° south of the present, and on the meridian of Greenwich—that is to say, midway between Greenland and Spitzbergen? and would not, eventually, the axis of rotation correspond in position with the axis of figure?

If the answer to these questions is in the affirmative, then I think it must be conceded that even minor elevations within the tropics would produce effects corresponding to their magnitude, and also that it is unsafe to assume that the geographical position of the poles has been persistent throughout all geological time.

It is not the first time that I have insisted upon this point; for some ten years ago* I pointed out another possible means of accounting for a change in the geographical position of the axis of the earth. My hypothesis was, however, founded on the assumption of the globe consisting of a comparatively thin crust, with an internal fluid nucleus over which the crust would slide when, from any geo-

* Proceedings of the Royal Society, 1866.

logical cause, its equilibrium was disturbed. To this it has been objected*—1st, that there would be a tendency in the transfer of sediment from one part of the globe to another, and in the various elevations and depressions of land simultaneously, to balance each other; and, 2nd, that the friction over the nucleus would be too great, and that, owing to the earth being a spheroid and not a perfect sphere, any motion of the crust would be attended by great resistance, and the bending and rending of its mass.

To these objections it may be replied that the effects of the transfer of sediment from one place to another, and of elevations and depressions of land going on at the same time, are just as likely to be doubled by the depressions taking place in the same hemisphere as the elevations, but on opposite sides of the Pole, as they are to neutralize each other; and, 2ndly, that with a comparatively thin crust, the readjustment to a fresh position on a nucleus so slightly spheroidal as that supposed to exist in the earth, is not accompanied by any great change of form, or certainly not more than what the contorted rocks all over the world have undergone.

I am not, however, on the present occasion, going to attempt to prove that the assumption involved in my hypothesis is reasonable. How we are to account for all the vast oscillations of the earth's surface, which we find to have been going on ever since the earliest geological period up to the present day, on any assumption more reasonable, I will leave for others to determine. I have already called attention to the bearing which recent researches in Solar Physics have upon this subject; and I am content to leave the matter as it stands, in the hope that before many years have passed we may learn more either in its proof or disproof.

The moral which I wish to draw from all that I have just said is this:—that so long as there is a possibility, not to say a probability, of the geographical position of the Poles having changed, it is premature to invoke intense glacial periods to account for all the glacial phenomena which may be observed. Much as we must esteem the labours of M. Adhémar and Mr. Croll, and others who have gone so deeply into the question of glaciation—enormous as have been the effects of ice in this and other countries—there are many who cannot but feel that the ice-caps invoked almost transcend our powers of belief, and who will be grateful to any astronomer or mathematician who will bring the pole round which they were generated somewhat nearer to our doors.

* Lyell's 'Principles,' 11th edition, vol. iii. p. 209.

There is yet one point on which, before quitting the subject, I may add a few words. Sir J. W. Lubbock, in the paper from which I have already quoted so much, has hinted at the possibility of some want of homogeneity in the constitution of the globe, so that, in cooling, the position of the axis of rotation may have changed. The varying amount of subterranean heat and volcanic energy in the same region at different periods of the earth's existence has frequently been commented on, as has also the varying degree of subsidence or elevation in the same tract at different times. The forces, whatever they may be, to which these upward and downward movements are due, have, as Sir Charles Lyell has remarked, "shifted their points of chief development from one region to another, like the volcano and the earthquake, and are all, in fact, the results of the same internal operations to which heat, electricity, magnetism, and chemical affinity give rise."

Whether changes in the specific gravity of enormous masses of rock in consequence of their being heated would be of sufficient degree to disturb the equilibrium of the globe, is a difficult question; but the remarkable position of the magnetic poles of verticity with regard to the actual poles of the earth, and the distribution of the magnetic force over the earth's surface may, as has been suggested to me by Capt. F. J. Evans, F.R.S., have some geological significance. These poles are in lat. 70° N., long. 96¾° W., and in lat. 73½° S., long. 147½° E. If we draw a circle around the globe, cutting these two points, we find that the magnetic poles, instead of being 180° apart, are only about 165° distant in one direction, while they are about 195° in the other. In like manner the magnetic equator, or line of no dip, differs considerably in position from the terrestrial equator, being drawn about 15° to the S. over South America, and about 10° to the N. over Africa and in passing the great Asiatic continent. There is also this singular circumstance, which was insisted upon by Sir Edward Sabine nearly forty years ago—viz., that if the globe be divided into an Eastern and a Western hemisphere by a plane coinciding with the meridian of 100° and 280°, the Western hemisphere, or that comprising the Americas and the Pacific Ocean, has a much higher magnetic intensity distributed generally over its surface, than the Eastern hemisphere, containing Europe and Africa and the adjacent part of the Atlantic Ocean. The points of the greatest intensity of the magnetic force, moreover, do not correspond with the magnetic poles, as there are two such foci in the northern hemisphere (those of Ame-

rica and Siberia) making it probable that there are two also in the southern hemisphere.

Such facts would seem more in accordance with a want of uniformity in the inner constitution of the globe than with there being perfect symmetry in the arrangement of all its component parts. Some abnormal features in the direction of gravity in different parts of the world seem also to afford corroborative evidence to the same effect. The subject is one of perhaps too theoretical a character for the geologist to approach; but if any definite connexion could be established between terrestrial magnetism and the internal constitution of the globe, we might, possibly, be justified in drawing the inference from its phenomena, that there are forces in operation in the interior of the earth by which its equilibrium may have been disturbed, and its axis of revolution thus caused to change in position.

It is, however, time to turn to more general considerations.

With regard to stratigraphical geology, the main foundations are already laid, and a great part of the details filled in. The tendency of modern discoveries has already been and will probably still be to fill up those breaks which, according to the view of many though by no means all geologists, are so frequently assumed to exist between different geological periods, and to bring about a more full recognition of the continuity of geological time. As knowledge increases, it will, I think, become more and more apparent that our existing divisions of time are, to a considerable extent, local and arbitrary. But, even when this is fully recognized, it will still be found desirable to retain them, if only for the sake of convenience and approximate precision.

Just as with our ordinary reckoning of time, we divide our day not only into the larger and more natural periods of morning, noon, afternoon, and evening, but also into the minor and artificial subdivisions of hours, minutes, and seconds, so, with geological time, we divide it into Primary, Secondary, Tertiary, and Quaternary Periods, or by whatever other names we may call them—and then subdivide these into various minor periods, represented by the different formations. We shall, I think, eventually more fully recognize that, as is the case with the periods of the day, each of the larger geological divisions follows the other without any actual break or boundary, and that the minor subdivisions are, like the hours on the clock, useful and conventional rather than absolutely fixed by any general cause in Nature. Had not the ancient

Assyrians been sufficiently civilized to have got beyond the decimal system, we might have had ten hours in our day and a hundred minutes in our hour; and had the earlier geological investigations taken place elsewhere than in Western Europe, we might, not improbably, have found the main divisions of geological time as well as its subdivisions somewhat differently placed from where they are at present, and are ever likely to remain.

With regard to Palæontology, though accuracy of observation will ever remain absolutely indispensable, I venture to prophecy that the great work of future Palæontologists will rather lie in still further developing the affinities of genera, than in merely recording the minute distinctions of species. The discoveries which have of late been made have a tendency to fill in the missing links in the chain of organic nature, and to lead to the adoption of some form of that great doctrine of evolution which has received so large an amount of support from a former occupant of this Chair, to whom we have this day presented the Wollaston Medal, Professor Huxley. It is highly probable that much more will be done in the same direction. In addition to what has been effected by Mons. Albert Gaudry in his researches on the fossils of Pikermi and Mont Léberon, and by Dr. W. Kowalevsky in his investigations of the osteology of the Hyopotamidæ, the discoveries of Professors Marsh and Leidy in America are doing much towards illustrating the line of descent of many of the higher mammalia. The highly important paper communicated to us by Prof. Owen at our last meeting gave, moreover, no uncertain sound as to the probable affinities between some mammalian and reptilian forms. On the other hand, the *Odontopteryx toliapicus*, described by Professor Owen in the pages of our Journal, and the *Ichthyornis* of Professor Marsh, and his *Hesperornis*, with teeth in both jaws (not in sockets like those of *Ichthyornis*, but in grooves, as is the case with *Ichthyosaurus*), support most strongly the conclusions of those who had already recognized the affinity of birds and reptiles, and lead me to hope that speculations in which I indulged some thirteen years ago with regard to the *Archæopteryx* having been endowed with teeth may eventually be confirmed. I will not attempt to enlarge upon a subject with the details of which I am so imperfectly acquainted. A very small amount of knowledge, however, suffices to convince an impartial observer of the great probability of all the vertebrate forms of the present day and those found in the later geological deposits, being the direct descendants of those of earlier periods.

If, as is now undoubtedly the case, we are able to trace the exact

pedigree of certain vertebrate forms, it is impossible to doubt that various invertebrate forms must in like manner be derived by direct descent from others previously existing. How far, however, the pedigree of some of the lower forms of organic life may be carried back in geological time is a question for the future, on which, no doubt, investigations in new fields of research will ere long throw light. Judging from what is already known, it seems highly probable that we shall, sooner or later, find traces of life in rocks which have hitherto been regarded as azoic, or, indeed, are as yet unknown.

Should the earlier forms from which the Crinoids, Asteriadæ, Mollusca, and Crustacea of the Silurian age, the fishes of the Old Red Sandstone, and the Saurians of the Triassic and Mesozoic strata are descended be eventually discovered, it will cause less surprise to many biologists than did the assertion, by so accomplished an observer as the President of the Geological Section of the British Association at the Bristol meeting (Dr. Wright), that "Palæontology affords no support to the hypothesis which seeks by a system of evolution to derive all the varied forms of organic life from preexisting organisms of a lower type."

With regard to geological progress during the past year, I must first express my regret that, owing to the illness of Mr. A. Everett, who, as I mentioned last year, had kindly undertaken the examination of some of the ossiferous caverns of Borneo, nothing has been done in that country. The continued examination of Kent's Cavern has not brought any material new facts to light; but that of the Settle Cave seems to have confirmed those who have been engaged upon it in the belief that it affords evidence of the existence of man in this part of the world before the great ice-sheet of the Glacial Period, and before the last great submergence of the country. In connexion with this case, there is another interesting point to which my attention has been called by Mr. Tiddeman, the separation of the fauna containing the Reindeer, and presumably significant of a cold climate, by a thickness of twelve feet of laminated clay, stalagmite, &c. from that containing the Hippopotamus, which may be regarded as characteristic of a warmer climate. In connexion with climate, however, it must be borne in mind that the Reindeer lived down to historical times in Scotland. The human fibula, which has been determined by Professor Busk, was found in the lower deposits associated with bones and teeth of *Elephas antiquus*, *Rhinoceros hemitœchus*, and *Hippopotamus*. It is to be hoped that the further

investigations still being carried on may lead to the discovery of other human relics.

The first sub-Wealden boring, from which such great hopes were entertained, has had to be abandoned; but, owing to the enterprise of Mr. Willett and the Diamond Boring Company, a new bore-hole has been sunk, which is still in progress. The depth attained is 1875 feet; but the work has lately been interrupted, owing to its having been found necessary to reline the bore. This second boring has served to correct the opinion that the Oxford Clay had been reached at a depth of about 1000 feet. It now appears that the fossils discovered down to a depth of about 1760 feet are such as have been hitherto considered to denote the Kimmeridge Clay of England and of the Continent. The boring is now in what seems to be some member of the Oolites, possibly the Coral Rag; and it yet remains to be seen whether any Palæozoic rock will be reached within the limits of 2000 feet, to which it is proposed to carry on the work. Whatever other results may accrue, it is something to have ascertained that in the centre of the Wealden area there is so large and unexpected a development of the Kimmeridge Clay. Competent judges estimated that its probable maximum thickness at the spot would not exceed 800 feet, but it is actually of more than double the estimated thickness. Such a fact appears to me to suggest that, at all events in some cases, areas in which the greatest elevation have taken place, may also be those which have been equally favourable to subsidence, and that where are now the centres of domes of elevation may at some former period have been the deepest part of basins of depression. If this has been the case in the Wealden area, it would seem probable that it is near its margin rather than at its centre that we should expect to find the older rocks most nearly approaching the surface. Such a speculation, however, unless corroborated by other examples of the same kind, cannot be accepted as of much value.

With regard to the memoirs communicated to the Society during the past year, I need say no more than that they have been numerous and, in many cases, of great interest and importance. The Abstracts of our Proceedings, which are now issued promptly after each meeting, will have enabled all of our Fellows to judge of what has been going on; and it would only be to waste your time to attempt to give any other summary. We have had Geological papers relating to all the periods, from the Cambrian down to the Historical, and Palæontological memoirs of the highest importance;

nor have the departments of Physical, Chemical, and Microscopic Geology been neglected.

Of the Geological books which have appeared during the year, it is hardly my province to speak; but I think the Society may be congratulated on the publication of the first volume of the 'Geological Record,' under the auspices of Mr. Whitaker, the yearly volumes of which, as containing an account of all the works on Geology, Mineralogy, and Palæontology published during the previous year, cannot but be of the greatest service.

There is only one more subject on which I will say a few words, and which, as to some slight extent involving a question in which I am personally interested, I have kept for the end of my Address.

It is one to which it appears probable that the earnest attention of geologists will immediately be called—namely, the water-supply of this vast metropolis. This is, indeed, not the first time that the attention of this Society has been called to it; for Professor Prestwich devoted to it a considerable portion of his Presidential Address in 1872. It has since been more fully discussed in the Sixth Report of the Commissioners appointed in 1868 to inquire into the best means of preventing the pollution of rivers, who have extended their inquiries somewhat beyond what appear to be the strict limits of their Commission. It is with their Report that I am mainly concerned.

The Commissioners have expressed their opinion that the rivers Thames and Lea (or Lee, as the word is spelt in their Report) ought to be abandoned as early as possible, and especially the former, as sources of supply to London. They regard the condition of these rivers as hopeless, and point out that an abundance of spring- and deep-well water can be procured in the basin of the Thames and within a moderate distance of London; and they are further of opinion that the metropolis and its suburbs should be supplied, on the constant system, exclusively with this palatable and wholesome water.

They believe that within 40 miles of St. Paul's a sufficient volume of deep-well and spring-water can be obtained for the present daily wants of the metropolis, but especially point to the Chalk and Upper Greensand above the Gault as the sources of supply. They state that within 30 miles of London there is an area of 849 square miles "covered" by these formations, and that within 40 miles radius the area is 1597 square miles.

They estimate (to a great extent guided by experiments carried on during many years under my superintendence) that the portion of the annual rainfall upon this large extent of porous rock, which sinks to reappear in springs and streams, may be taken at 6 inches annually, and point out that this amount of infiltration into the Chalk area within 30 miles of the metropolis indicates the quantity of 202 millions of gallons daily as the theoretical maximum supply available from that area. They suggest that the greater portion of this water, which now escapes in springs and in the river-beds at the lower levels of the absorbent district on which it falls, might be abstracted by a sufficient number of wells sunk below the present spring-heads of the district, and so constantly drawn upon, that there should always be a void for the reception of unusual rainfalls below the level at which the drainage of the district naturally escapes. They incidentally admit that any water drawn from the subterranean reservoir in the Chalk by artificial means will be at the expense of the streams which now flow through the valleys in the Chalk area, but do not give even a passing consideration to the effect upon that area of abstracting from it its natural supply of water, and conveying it—"convey, the wise it call"—to London, should the scheme they advocate ever be carried into effect.

It can hardly be believed that a proposal such as this, involving the diversion of the whole of the water from the natural springs and streams over an area of not less than 440 square miles (an area larger than that of several English counties), should have been brought forward without the slightest reference to what would be the result upon this vast extent of country, the inhabitants of which are to be sacrificed to the presumed needs of this overgrown city. It will, I think, come within the province of the geologist to point out not only where spring-water of good quality is to be obtained, but also what will be the effect of its abstraction upon the districts where it now exists in sufficient abundance to overflow into the streams. It will be for him to show what will be the effect of producing "a void below the level at which the drainage of the country naturally escapes;" how what are now fertile and even irrigated meadows will be converted into arid wastes; how watercress-beds, now of fabulous value, will be brought to the resemblance of newly mended turnpike-roads; how in such a district all existing wells, many of them already some hundreds of feet in depth, will be dried, the mill-streams disappear, and even the canals and navigable rivers become liable to sink and be lost in their beds. And

these results would, if the scheme were carried out, not be confined to some single spot, but would extend over hundreds of square miles.

It may perhaps be thought that I am exaggerating the size of the area, the natural water-supply of which it is proposed to abstract but the calculation may be readily verified.

The quantity of water now daily supplied to London by the different water-companies, exclusive of the Kent Company (which already supplies deep-well water to the extent of 9,000,000 gallons daily) is stated to be 104,800,000 gallons. Now if the supply of 6 inches of rainfall per annum, absorbed over 849 square miles, be, as the Commissioners calculate, equivalent to 202,000,000 gallons daily, it is evident that it will require more than half that area to furnish 104,800,000 gallons daily, the exact figures being 440½ square miles.

It must, however, be remembered that the Commissioners regard this quantity as the theoretical maximum of water-supply available from such an area. And they are right in so doing; for in some years a far larger area would have to be exhausted in order to produce so large a water-supply, since not unfrequently the quantity of the rainfall which percolates to a depth of only 3 feet into the soil, instead of being 6 inches, as supposed in the calculation, is as low as 3 inches. For three years running I have known the percolation through a depth of 3 feet of ordinary soil covered with vegetation to have been on the average only 3½ inches, and through Chalk under the same conditions less than 5¼ inches. It would appear, then, that it would be safer to regard the available spring-water supply as not representing more than 4 inches of the rainfall per annum, instead of 6 inches, in which case the area requisite to supply 104,800,000 gallons daily would be 660 square miles.

To avoid any possible error, let us look at the matter from another point of view. One inch of rain falling over a statute acre produces, as nearly as may be, 100 tons or 22,400 gallons of water. Dividing this by 30, as representing the daily consumption of one person, there would be enough for one person for 743 days, or, say, for two for one year. Four inches of rain would render each acre capable of supplying the wants of eight persons, so that a square mile of 640 acres would supply 5120 persons for one year. Calling the population of the metropolitan area 4,000,000, and dividing that number by 5120, we arrive at an area of 780 square miles as necessary for their supply.

There can therefore be no doubt as to the vast extent of country which the proposal of the Commissioners would place under unnatural conditions with regard to its springs and streams.

No doubt wells may, in some few instances, be placed in such a position as to affect but slightly the neighbouring streams. The wells of the Kent waterworks, for instance, which supply 9,000,000 gallons daily, are so placed as mainly to derive their supply from water that would otherwise find its way into the Thames by springs along its bed; indeed, from the amount of chlorine present in the water, it may be doubted whether some portion of it is not derived from the Thames itself by filtration through the chalk. It seems probable that in the valley of the Thames immediately above London there may be spots from which a limited supply of water might be pumped without much injury to the neighbouring property; but a wholesale abstraction of the entire supply of spring-water from an area of even 300 or 400 square miles could not be otherwise than most disastrous.

On looking at the actual chemical analysis of the waters supplied by the different Companies, as furnished by the Commissioners, there would, at first sight, appear to be some difficulty in understanding their reasons for so highly commending the Kent Company's water, and so unhesitatingly condemning that of the other Companies, if we are to take as our guide the "previous sewage or animal contamination," on which so much stress is laid. It is hard to comprehend why, if river- or flowing water which exhibits any proportion, however small, of "previous sewage or animal contamination" is to be regarded as suspicious or doubtful, the water in wells, say 100 feet deep, may be allowed 10,000 pints in 100,000, or 1 pint in 10, and may yet be regarded as reasonably safe. For in these deep wells, if at no great distance from a river such as the Thames, it by no means follows that there is not some amount of comparatively direct communication through which water may trickle rather than filter; and not improbably the river-water below London is more objectionable for drinking purposes than it is higher up the Thames.

Let us for a moment compare the "previous sewage or animal contamination" of the water supplied by the different Companies deriving their water from the Thames and Lea with that of the Kent Company's water. I take the average of the different analyses of each, as given at p. 270 *et seqq.* of the Report.

West Middlesex	3·083
Grand Junction	3·226
Southwark and Vauxhall	2·983
Lambeth	3·081
Chelsea	2·785
New River (excluding 1868)	2·751
East London	2·304
Average	2·888
Kent Company	6·480

or upwards of twice that of any one of the other Companies.

In this average, however, is included the water from the wells at Charlton and Belvedere, both of which are condemned by the Commissioners. Omitting these two, the average is 3·780, which is still far higher than any of the others.

If we refer to the headings Organic Carbon and Organic Nitrogen, there can be little doubt of the superiority of the Kent Company's water; but judging merely from the statistics under the awful heading of "Previous Sewage Contamination," that of the River Companies seems the purest.

Why the source of supply from the two rivers should be condemned as hopeless it is very hard to determine. This startling recommendation to give up the supplies of water on which London for centuries has depended, is brought forward just at a time when the most strenuous efforts are being made to purify the rivers Thames and Lea, and but a few years after the Commissioners on the Water Supply of the Metropolis, within whose proper sphere this question lay, had reported that with perfect filtration and efficient measures taken for excluding from them the sewage and other polluting matter, these rivers will afford water which will be perfectly wholesome and of suitable quality for the supply of the Metropolis.

It is not for me to enter into the chemical part of this question; but I may venture to express a doubt whether considerably more might not be done by increased reservoirs for subsidence, and by artificial aeration of the water, in addition to filtration, so as to carry still further the oxidation of any organic matter it may chance to contain.

I have less hesitation in strongly insisting on the fact that, irrespective of the New River water, the metropolis is already supplied with 9,000,000 gallons per diem, or at least 2¼ gallons per head,

of the deep-well water so highly commended, a quantity which would seem amply sufficient for dietetic and culinary purposes. I am, moreover, of opinion that the difficulty of distributing this water over the whole area by means of a second service distinct from that of the water for ordinary domestic purposes, though great, is by no means insurmountable. Even were the waters of the Thames and Lea unfit for drinking purposes, it is very far from being the case that London is in the same plight as Coleridge's 'Ancient Mariner,' with

"Water, water everywhere,
Nor any drop to drink."

Enough is already there for all culinary and dietetic purposes, could it but be distributed; and to lay out incalculable sums of money and inflict incalculable mischief, in order to import chemically pure water with which to lay the dust in our streets, and to flush our sewers, seems "a multiplying improvement in madness, and use upon use in folly." We might almost as well import wine for the purpose; and in that case we might find an historical parallel in the proclamation of Jack Cade:—"Here, sitting upon London Stone, I charge and command, that of the City's cost, the conduits run nothing but claret wine the first year of our reign."

As deeply interested in the water-power and general prosperity of one of the Chalk valleys within the fated radius of 30 miles, I may have spoken strongly on this question, and may not unfairly be accused of having done so from interested motives. No one, however, can submit silently to an insidious attack upon the property which he is fairly entitled to hold; and after carrying on experiments, for upwards of twenty years, as to the percolation of water to the underground springs in a Chalk area, I may claim some experience in such a question, and much doubt whether my judgment is seriously distorted. Even should the abstraction of water be spread over a much larger area than has been supposed, so as to reduce the amount conveyed away from any particular district, or even should the gross quantity required prove less than supposed, it may be left to any one who will take the trouble to investigate the matter to determine whether the effects if wider spread, or somewhat diminished in intensity, would be much less injurious. Any injury from this cause would moreover be felt with double intensity at those seasons, which are of by no means unfrequent recurrence, when, even without this gigantic artificial abstraction, the supply of water in the upper portions of the Chalk district be-

comes short, and wells which during the previous season may have had 50 or 60 feet of water in them run absolutely dry.

It now only remains for me to thank the Council, the Officers of the Society, and the Fellows at large for the uniform kindness and consideration which they have extended to me, not only during the two years I have had the honour of being your President, but during the eight preceding years, during which I was one of your Secretaries. I look back with pleasure on the prosperity which, during those ten years, the Society has enjoyed, a prosperity which I hope may continue even in a greater degree now that I quit this Chair in favour of my old friend and fellow Secretary, Professor Duncan, who is, in all respects, so thoroughly well qualified to fill it.